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ABSTRACT

It is argued that when a discipline, such as human geography, overemphasizes order and neglects skepticism and criticism, the products of research are likely to be erroneous, misleading, and self-fulfilling in nature. The first three chapters present the core of the argument. Chapter 1 establishes the case for an order-skepticism dialectic. Chapter 2 discusses why much recent research in human geography has emphasized order. Chapter 3 suggests some social and institutional barriers to skepticism and criticism. Chapters 4, 5, and 6 comprise three case studies of research in human geography that illustrate the ways in which order is emphasized, with unfortunate results. The case studies show how geographers have uncritically accepted the concepts of hexagonal market areas, S-shaped diffusion curves, and rank-size relationships among urban places. The concluding chapter provides some conclusions and preliminary suggestions for improving the place of skepticism within human geography. A bibliography is provided. (RM)

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ORDER AND SKEPTICISM:

Human Geography and the Dialectic of Science

Richard Szymanski
Bodie, California

and

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SP 015 001

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Foreword

It is appropriate that during the inaugural year of the **Resource Publications in Geography,** a volume is addressed to Anglo-American geographers as practicing social scientists. In this book, Richard Szymanski and John Agnew ask us to examine our work at two levels. The first embraces how we go about being geographers — how we work, develop hypotheses, build models, test theories, communicate with one another, and educate our students. Szymanski and Agnew argue that we conduct ourselves too uncritically. Reasons can be found, they propose, in the nature of our discipline and in our interaction as a community. They suggest that in research and, particularly, in teaching, we are insufficiently skeptical about the geographical regularities, the theoretical spatial order, we report existing in the world around us.

The second level of analysis in this book invokes but three of many theoretical structures created and perpetuated in geographic research and teaching: hexagonal market areas, S-shaped diffusion curves, and rank-size relationships among urban places. By examining the uncritical acceptance of these concepts by many geographers, Szymanski and Agnew both support their primary level of discussion, directed at how the discipline functions, and provide a second level of concern, that of critical examination of particular geographic ideas. Certainly important themselves, these analyses nevertheless bring to mind numerous other "truths" that appear in our textbooks and persist in our research. The series editor and advisors hope this book will stimulate similiar re-examination of other enthroned geographic truths that *may* be revealed as pretenders!

This book serves as a mirror of ourselves, an image of how we might proceed. If, as Szymanski and Agnew argue, a healthy discipline practices criticism and skepticism as intrinsic to scientific discovery, then their work contributes to a dialectic of order and skepticism in human geography. This book itself should also be viewed critically, an affirmation of a constructive approach toward progress in the field. To do otherwise would be to substantiate the authors' claims that we are insufficiently critical.

Resource Publications in Geography are sponsored by the Association of American Geographers, a professional organization whose purpose is to advance studies in geography and to encourage the application of geographic research in education, government, and business. This series traces its origins to the Association's Commission on College Geography, whose *Resource Papers* were launched in 1968. Eventually 28 papers were published under sponsorship of the Commission through 1974 with the assistance of the National Science Foundation. Continued NSF support after completion of the Commission's work permitted the *Resource Papers for College Geography* to meet the original series goals for an additional four years and sixteen volumes:

> *The Resource Papers have been developed as expository documents for the use of both the student and the instructor. They are experimental in that they are designed to supplement existing texts and to fill a gap between*

significant research in American geography and readily accessible materials. The papers are concerned with important concepts or topics in modern geography and focus on one of three general themes: geographic theory; policy implications; or contemporary social relevance. They are designed to implement a variety of undergraduate college geography courses at the introductory and advanced level.

The popularity and usefulness of the two series suggested the importance of their continuation after 1978 once a self-supporting basis for their publication had been established.

For the **Resource Publications,** the original goals remain paramount. However, they have been broadened to include the continuing education of professional geographers as well as communication with the public on contemporary issues of geographic relevance. This monograph was developed, printed and distributed under the auspices of the Association, whose members served in advisory and review roles during its preparation. The ideas presented, however, are the authors and do not imply AAG endorsement.

For our colleagues, we hope that this book reinforces both the spirit and the practice of a science that views discovery and criticism, order and skepticism, as an inherent dialectic. For our students, we trust that this volume will be meaningful in their education and in development of a constructively critical posture toward "truth."

C. Gregory Knight
The Pennsylvania State University
Editor, Resource Publications in Geography

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Preface and Acknowledgements

Much of the history of philosophy and the history of science can be seen as a debate between the claims of order and those of skepticism. One writer, Popkin (1960), has chosen to view the history of philosophy in Europe during the immediate post-Reformation period entirely in terms of struggle with the concept of skepticism as inherited from the Greek skeptics. The history of science can be seen in like manner. The work of great scientists such as Darwin and Einstein clearly involved a skeptical pursuit of order. Their pursuit was skeptical in that underlying beliefs and order-claims rarely went unquestioned. Science for them was thus more a *process* of skeptical reasoning than a set of *products* — facts, assumptions, generalizations, laws, and theories. In usual scientific practice, however, order has often received much more attention than skepticism. This book is about how contemporary human geography has come to value order over skepticism, and why this is an unhealthy state of affairs.

Our aim is to draw attention to the dialectical interplay between a researcher's most important principle — a belief in order — and skepticism. In particular, the book attempts to show that when a discipline overemphasizes order and neglects skepticism and criticism, the products of research are likely to be erroneous, misleading, and self-fulfilling in nature. We know of no study that has been directly addressed to this issue, particularly in a specific discipline. Individual human geographers such as Hartshorne (1939) and James (1967) have hinted at the importance of skepticism and criticism. But the issue of an order-skepticism dialectic has never received extended treatment in geography or elsewhere.

Although the discipline we use is Anglo-American human geography, the argument should be of interest to all social scientists. Many of our examples have general interest, and some of the literature we cite is from sociology, economics, political science, anthropology, biology, and statistics. The argument also has generality in that it can be applied to substantive research topics, methodologies, and philosophies of research other than those claiming the mantle of science. Part of our belief is that all intellectual endeavor can be viewed in terms of an order-skepticism dialectic.

The first three chapters lay out the core of the argument. Chapter One establishes the case for an order-skepticism dialectic. Chapter Two discusses why much recent research in human geography has emphasized order. Chapter Three suggests some social and institutional barriers to skepticism and criticism. Chapters Four, Five, and Six comprise three case studies of research in human geography that illustrate the ways in which order is emphasized, with unfortunate results. Finally, Chapter Seven provides some conclusions and preliminary suggestions for improving the place of skepticism within human geography. In the case studies and concluding chapter reference is made to the perpetuation of an overemphasis on order through textbooks. This reflects our belief that habits of thought are acquired in the classroom as much as at the research frontier.

The authors became involved in the project upon which this book is based in different ways. Szymanski was indoctrinated with a peculiarly capitalistic interpretation of the order-skepticism dialectic while a practicing Certified Public Accountant in California and Chartered Accountant in Australia. One of his principal functions as a senior auditor was to uncover financial irregularities and fraud. Upon entering that disciplinary house called geography he assumed that its practitioners nurtured a variant of what he had come to accept as second nature. Experience in five North American geography graduate departments and a not uncommon negative reaction that questioned the propriety of writing one particular critical paper (Symanski 1976) conclusively revealed his naiveté. In short, he discovered to his dispair that geography accepts the primacy of harmonious social relations ("Don't rock the glass canoe" was a song he encountered all too often). Thus the genesis of this book. Finally, on an immensely more cheerful note, it must be said that the best of Szymanski's ideas in what follows was cultivated while oystering and roistering with Nancy in a congenial ghost town.

Agnew became involved because, like Szymanski, he was concerned that human geography and most social sciences have lost connection with the tradition of the Enlightenment that self-consciously confronts claims of order with a skeptical outlook. He believes that this has happened because of a lack of interest in philosophical questions and an obsession with data rather than theories on the part of social scientists. A fundamental belief is shared by both authors. In Ryle's words: "The rationality of man consists not in his being unquestioning in matters of principle but in never being unquestioning; not in cleaving to reputed axioms, but in taking nothing for granted" (quoted in Popper 1976:125).

Some terms used in the book — such as positivism, idealism, realism — have specific definitions and complex histories in the philosophy of science. Although we offer definitions when appropriate, readers are urged to consult *The Encyclopedia of Philosophy* to gain some sense of how such terms have been used.

A number of people have kindly read part or all of the manuscript in various stages of preparation. We would particularly like to thank S.J. Baillie, R.J. Bromley, Nancy Burley, George Carey, Daniel Doeppers, Roger Downs, Gordon Ewing, Leonard Guelke, Greg Knight, David J. Robinson, Robert Sack, R.H.T. Smith, Dick Walker, and Mike Webber. The authors, of course, are solely responsible for what they have made of the valuable comments provided by these friends and colleagues.

Richard Szymanski
John A. Agnew

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1

The Dialectic

Two seldom-discussed but pervasive principles of science are that the world is ordered but that one must be skeptical about data and explanations which purport to demonstrate order. The centrality of each of these differs from one discipline to another because of differences in subject matter, methodology, canons of evidence, and the way in which scientists are trained. Scientists within the same discipline, for reasons as much personal as social, give unequal weightings to the two principles. In addition, some see them as highly interrelated; others behave as if they had little relation to one another.

We wish to argue that the principles of order and skepticism should always be closely interrelated — when they are not, we are likely to have an unhealthy science. Order, as a guiding principle, is necessary because without it science would be a futile enterprise: models, laws, and theories would lose their meaning. We would be buried in a morass of facts. The idea of explanation or prediction would be nonsensical. Yet the pursuit of order is not enough, and it can often be too heady. Skepticism is needed to keep scientists honest. Along with criticism, it is a means of making adjustments for excessive and erroneous claims, precisely those that grow out of the relentless search for order.

Science as an intellectual process is dialectical. Order is its quest, its thesis. Skepticism is its antithesis. Truth or knowledge, predictive success, or that explanation which makes a good scientist feel comfortable, is an historically specific synthesis. Basic is the idea that to be negative is to be positive. As Marchand has noted in a different context, the "basis of dialectics is the discovery that *to negate is a positive act*, the only one fitted to the nature of human thought and the nature of things (which are finite and self-contradictory)" (Marchand 1978:106).

To understand the scientific dialectic is to know that good science envisions, and never forgets, that truth, understanding, prediction, and explanation are momentary. They contain the seeds of something different, sometimes something better. The dialectic, as we conceive it in scientific context, is not synonymous with progress, but it is how we achieve progress. Under ideal conditions, where philosophical stance and research practice are profoundly entwined, the scientific dialectic is ever-present, on-going, never-ending.

By drawing on material from geography, we propose to show how one discipline has become insensitive to the dialectical interplay of order and skepticism. In general,

geographers have given too much attention to the search for order and not enough to skepticism. We identify numerous reasons why geographers have recently given undue attention to the principle of order. Because skepticism has been grossly underplayed, doubtful claims have been accepted as received knowledge. Worst of all, unwarranted, misleading, and spurious statements have made their way into that medium where errors are most difficult to expunge — textbooks.

We have little doubt that the inattention given to skepticism originates in the very nature of social relations. For this reason we work from the premise that matters of knowledge, the domain of epistemology, cannot be separated from questions of sociology. The act of knowing and the paths to understanding are inextricably tied to our behavior as social beings. Scientists are members of society, but to the extent they are separated from the latter, the separation is a distinction of the same order as the distinction between knowledge and society. This leads us to see our material through a lens best described as socioepistemological. It is a type of analysis that others might prefer to see as one aspect of humanistic geography, "making geographers aware of their own extreme interpretations of science, and making scientists aware of the social and cultural factors involved in so-called objective research" (Entrikin 1976:632). Like humanists we are interested in analyzing the nature of our hidden assumptions. But beyond noting this intersection we make no effort to see parallels between what we have to say and the position of humanists.

Although we accept the generality that language and ideology impose their own kinds of order on the phenomenal world, we do not accept the view held by some that all truth is an illusion or that all ordering systems are equally valid. Successful predictions and explanations coherently embedded within larger systems of understanding are proof that all is not chaos. In short, we believe that it is not a fruitless venture to pursue "truth," however unfinished and narrow the quest will always be. It is within this context that we are committed to science.

From a different perspective our analysis and concerns overlap with those who probe the sociology of science (Kuhn 1962; Merton 1973; Cole and Zuckerman 1975), an area of inquiry all but ignored by geographers (W. Bunge 1961; Hurst 1973).

In order to give empirical support to our arguments we make use of illustrative examples and three case studies. One shows that major geography textbooks have glaringly misrepresented the singular validity as well as the generality of a type of central place hexagon. Another case study contends that the S-shaped curve, well-known in the social sciences and particularly common in diffusion studies, has illegitimately been used as an explanatory device. Finally, the third extended example argues that the meaning of the rank-size rule has also been misrepresented and misused, as in the other two cases, in the relentless search for order.

Order in Science and in Geography

The concept of order and the notion of an ordered world seem to have their origins in primitive thought. They recur with regularity not only in the fullness of what evolution is and what we mean by it, but also in the history of ideas (Glacken 1967:3). The search for order and classification are so significant and so commonly a part of human make-up that they are undoubtedly linked with the evolution of intelligence and rationality (Popper 1974: 120, 237).

Although we often see the search for order as something peculiar to the scientist, the quest is common to all humans. We benefit from perceiving and codifying order

because such processes facilitate understanding and prediction. Prediction assists in the attainment of personal security and is an everyday intellectual activity, not just an aim of science (Toulmin 1960: 108). Understanding is similarly important in everyday life. To be sure, the scientist's concept of order is different from the layperson's, not so much in the degree to which it is elaborated and institutionalized, but rather in the peculiar way it is codified in such concepts as explanation, model, law, and theory.

Order in Science

Despite the obvious significance of order to the scientist, those who write about the philosophy of science often do not identify the concept explicitly. The reason appears to lie both in agnosticism concerning the metaphysical and ontological postulates of science and in preoccupation with explanation, prediction, models, laws and theories as proper domains of concern. But the significance of the implied concept of order is also never missed, particularly by positivist writers (Brown 1968:26; Parsons 1951: 335; Osterberg 1976:49; Storer 1966:80-81). For example, Rescher (1970:130) says that prediction is the definitive task of science. Wartofsky (1968:134) contends that "the object of research is to discover laws, or invariant relations which hold between the facts of observation and measurement." The opening sentence of Walker's book on *The Nature of Scientific Thought* asserts that "the immediate purpose of scientific thought is to make correct predictions of events in nature . . ." and ". . . the ultimate purpose is the survival of man . . ." (Walker 1963:1). The noted philosopher of science, Mario Bunge, identifies ten metaphysical maxims of science. His way of pointing to the order principle is the maxim that "everything satisfies laws. . . . If there were not laws, we would not attempt to discover them and utilize them to explain, predict, and produce" (M. Bunge 1964:15-16).

Perhaps the biologist, George Gaylord Simpson, has put the case as forcefully as anyone:

> *Scientists . . . the one thing that they do not and must not tolerate is disorder. The whole aim of theoretical science is to carry to the highest possible and conscious degree the perceptual reduction of chaos. . . . In specific instances it can well be questioned whether the order so achieved is an objective characteristic of the phenomena or is an artifact constructed by the scientist. Nevertheless, the most basic postulate of science is that nature itself is orderly. . . (Simpson 1961:5).*

Order in Geography

Geographers who have discussed philosophical issues have, on the whole, followed the lead of certain philosophers of science. Humboldt and Ritter thought that the important second stage of research, after orderly description, involved that of subsuming one's material under cause and effect laws. A science which did not do so was "pigeon-holing" and engaging in "antiquarianism." They saw no fundamental difference between the methodological approach of the physical and social sciences (Wrigley 1965:4-5). William Morris Davis believed that there was order in nature, his theory of cyclical erosion being more than mute proof. Ellsworth Huntington, usually associated with his interest in climatic influences, espoused geographic use of one of the most important laws in science, that of Darwin's law of natural selection (Huntington 1924).

Philosophically, Whitbeck was a precursor of a later geographer, Schaefer. He argued that geography ought to be associated with economics in a "science of geonomics." The search for laws would be an obvious pursuit (Whitbeck 1926).

The so-called environmental deterministS had invisible ties to many of these geographers, but they went further (Lewthwaite 1966). In their eagerness to make a case for the universality of environmental influences, environmentalists did not appreciate the profundity of the ordering premise from which they worked, nor did they see that order search and skepticism — in a healthy and mature intellectual context — are linked concepts. Indeed, lack of skepticism may have been a more fundamental problem than lack of proper care in data analysis or oversimplification of complex problems (Martin 1951:8).

Hartshorne had an unusual view of science, and yet in his major works and later intellectual confrontations, he continually spoke to the issue of order (Hartshorne 1939; 1959). He did so when discussing the concepts of regional and areal differentiation and, more directly and memorably so, in historic debates over the nomothetic and idiographic (Schaefer 1953; Hartshorne 1959). Preston E. James recognized the historical importance of order as concept, even if he did not emphasize its pivotal role as metaphysical axiom in the scientific mind (James 1972:7-9, 117-119). Perhaps Zelinsky recognized more clearly than previous writers the central role of the principle of order. In an impassioned criticism of what he refers to as the Church of Science, he identifies five axioms upon which — among others — he claims the churchly edifice rests: causality; the solubility of all questions; final, perfect knowledge; universal validity; and total scientific objectivity (Zelinsky 1975:130-33). Although we do not agree with his interpretation of some of the axioms he lists, nor with the significance he gives to others, he does correctly note that:

> *Scientists are the creatures concerned with universalities, with the pursuit and testing of general laws, presumably applying to all the subsumed objects at any point in space and time. This is the ultimate official identity of science, even when the immediate agenda covers only the seemingly unique . . . (Zelinsky 1975: 127).*

Even humanist geographers, with their dissatisfaction with "scientific geography," have found a place for the principle of order. Among other things, Tuan claims that the humanistic geographer is an "intellectual middleman" and that one of his main roles is to take the "nuggets of experience as captured in art and decompose them into simpler themes that can be systematically ordered. Once experience is simplified and given an explicit structure, its components may yield to scientific explanation" (Tuan 1976: 274).

Before turning from an older to a newer tradition, it is worth making a distinction between particular and universal order, between the idiographic and the nomothetic. While our principal concern is with the latter, it is nonetheless true that those who see the world in unique terms, or as systems of unique particulars, also hold to a premise of orderliness. It is worthwhile to explore briefly what this means to those who claim to be interested in particulars, not only because it extends the generality of our argument but also because those who have argued that they proceed from an idiographic premise overlap to a greater degree with those who hold to a universalizing principle than is often suspected.

For some geographers who adhere to something other than a nomothetic (law-finding) position, it might be said that they search not for the lawful but rather for the rational; they attempt to understand pattern in a place rather than pattern similarity in all

places. They may not feel at home with what has been termed an "idealist philosophy" by Guelke (1974), but they would accept Guelke's view that locating the rationale behind a given action constitutes an explanation for such action and, thus, the patterns generated by it. If one finds a rationale, then one "understands", one has found order.

Many cultural geographers have long counted themselves among those opposed to a nomothetic point of view and to other tenets of science. Nevertheless, they have entertained an order premise — the working assumption that cultural traits are adaptive or functional. In a broad sense cultural geographers have had a somewhat interchangeable bag of concepts: the adaptive is the functional, and the functional is the rational. In its most vulgar form, "adaptation is taken to be anything that works" (Trimbur and Watts 1976:180).

The concept of cultural stability, by no means the same as homogeneity, is another pillar in the cultural geographer's particularistic conception of order. Sopher (1972: 322) has identified the concept — and the problem with it:

> *. . . culture occupance of an area is . . . transitory and usually plural, and the identification of culture areas by matching cultural distributions is . . . difficult. Perhaps geographers should turn their questions about culture around to ask under what conditions cultures and culture areas will appear to be stable, although to do away with the assumption of cultural stability would be to invite methodological difficulties.*

However insistent some cultural and humanistic geographers may be that they are concerned with the unique case or the uniqueness of place, both their philosophical pronouncements and much of their research belie the claim.

It is of little surprise that the positivists in geography, particularly those identifying themselves with the logical empiricist view of science as the search for laws among observable events, have given the most attention to the principle of order, the meaning of lawful statements, and approaches to theory building (Amedeo and Golledge 1975; Harvey 1969). Their implicit acceptance of the principle occasionally surfaces in explicit fashion as when Golledge and Amedeo (1968: 774) note that "acceptance of the ultimate existence of laws is predicated on accepting the principle of rationality or non-randomness in a universe." The principle of order is sometimes glaringly absent when geographers *qua* philosophers talk about scientific posturing within the discipline (Walmsley 1974). Entrikin, for example, exemplifies positivistic thinking when he sees "scientific geography" as based upon three beliefs: empirical observation, public verifiability of conclusions, and the isolation of fact from value (Entrikin 1977: 210). He discusses laws, models, and theories, but not the axiom upon which they are predicated.

Two of the most strikingly explicit efforts to recognize order as guiding principle are found in Haggett's *Locational Analysis in Human Geography* and in Abler, Adams, and Gould's *Spatial Organization*. Haggett's first chapter of "Assumptions" begins with a section entitled, "On the Search for Order" (Haggett 1966: 2-9). Abler, Adams, and Gould devote nearly eighteen pages to the concept, including identification of four major types of order: theological, aesthetic and emotional, common sense, and scientific (Abler, Adams, and Gould 1971: 4-21).

What appears to be lacking in all of these efforts to discuss modes of scientific analysis is mention of the potential dangers of the unrelenting commitment to order. Worse, perhaps, attention is not given to the concept of skepticism as an equally

fundamental principle. Therefore there is no mention of its pairing with the principle of order (Amedeo and Golledge 1975; Haring and Lounsbury 1971; Harvey 1969).

Dogmatism, Skepticism, and Criticism

In contemporary Anglo-American professional philosophy, the notion of skepticism is identified with the denial that one may know anything for certain. This is not the sense in which we envisage skepticism. Neither do we advocate the "anything goes" philosophy of epistemological anarchism with its playful defense of the outrageous and adolescent opposition to established truths (Feyerabend 1975). Rather, we accept the classical definition of skepticism, expressed by Naess (1968: 4), as the view of those who "neither claim that they have found at least one truth nor claim to *know* that truth cannot be found, but persist in their seeking." Thus skeptics are to be distinguished from "dogmatists," those who claim to have found at least one truth, as well as from "academicians," those who deny the possibility of knowing anything for certain. Naess (1968:56) argues that the skeptic,

> *just like anyone else, can be misled by illusions, note his mistakes, and correct them. He simply suspends judgment in relation to any proposition claiming to say something* true *about how things really are. Reality is in darkness, but not necessarily in darkness; perhaps it can be brought to light; at least he (the skeptic) does not know that it cannot. In fact, the skeptic himself may, in his own opinion at least, be the one to bring it to light; he may discover how things really are.*

This position is not meant to imply that the skeptic has to feel an obligation to doubt more than others (Sextus Empiricus 1933). "Indeed, the urge to doubt and seek should apply more to the dogmatist, for . . . the more one postulates . . . the more there is to doubt" (Naess 1968:53).

Philosophical pronouncements provide us with a backdrop, some suggestions on the broader meaning of skepticism. But further distinctions are required. In particular, it seems useful to differentiate between primitive and systematic forms of skepticism and between parallel concepts of criticism.

Skepticism can be envisioned as existing on a continuum, from doubt which is primitive to that which is systematic. Primitive skepticism is highly unfocused. One feels or senses that a given scientific finding is not right or true, though without knowing why or how strongly one feels such doubt. This kind of skepticism need not, indeed, often does not, go any further; it remains a raw, almost intuitive, feeling that cannot or can only barely be articulated. Primitive skepticism occurs before the systematic, and is therefore more fundamental. In short, it is the first, spontaneous mental signal that something is not quite right. By contrast, systematic skepticism is more focused, more easily articulated, more certain as to degree of doubt. The difference between primitive and systematic skepticism can be seen in the way scientists react toward research. One expresses primitive skepticism when he says, "Something is wrong here," or "This doesn't look quite right to me." Systematic skepticism is evident when one says: "I doubt that the statistic you used to test that relationship was the correct one," or "Do you really think that the relationship is linear? It appears to be curvilinear."

Criticism is more definitive than skepticism, though not necessarily more focused. It can also be an activity separate from an actual piece of research. Criticism is an extension of doubt to the point when one says: "This is the wrong statistic to have

applied under the circumstances," or "This is a poor statistic to have used; a better one would have been . . ." Both of these examples of criticism can be seen as systematic in that they point to something definite within a piece of research. That the criticism might be wrong or ill-founded is not of particular importance; such a determination is a matter of weighing the criticism against the piece evaluated.

Primitive criticism is akin to primitive skepticism in that it is unfocused; it is a kind of blanket condemnation, or one not specific enough for someone to sensibly weigh. It takes the form, "This is a second-class research effort," or "Why anyone would have researched such trivia is beyond me." Primitive skepticism is often healthy. If it cannot be further developed into a systematic variety and articulated by the original skeptic, someone else may, as a result of knowing of another's skepticism, take a closer look at the alleged findings. Primitive criticism is usually unhealthy and destructive, impeding rather than enhancing scientific progress. This is because it purports to be a value judgment about the quality or significance of research when, in fact, it is no such thing. It is too unfocused to be of any utility; the charges cannot be interpreted in any meaningful way by the person whose research is the object of attack; and the criticism cannot be elaborated or expanded upon by the critic.

Primitive criticism is inherently no more destructive than primitive skepticism, and in certain contexts there is no difference between the two. That primitive criticism is more frequently destructive than is primitive skepticism arises initially from the fact that it is more assertive, secondarily from social context. Criticism in general is socially unpalatable, and more so if it is pointed. By definition, the primitive form cannot be used in any meaningful way in a scientific context and often hampers future intellectual exchanges. Thus, primitive criticism is destructive and lacks scientific utility.

It is conventional to think of skepticism and criticism as either destructive, or, for some, as both destructive and constructive. The apparent pejorative meaning of criticism led one geographer more than two decades ago to write an article to the effect that the words "criticism" and "critics" should be replaced by "discussion" and "discussants" (Whittlesey 1954). Our position is that all but what we have labeled as primitive criticism is essentially constructive. That is, primitive and systematic skepticism and systematic criticism are positive. If they are unwarranted, then the questioned findings, truths, and whatever will — in the long run — remain essentially as they were before being questioned. If skepticism and criticism are well-founded, then there is a likelihood that the existing claims or scientific propositions will be altered or abandoned. Should such change occur, the edifice of science as product will be smaller or different, cleaner, or truer to its fundamental quest. Because knowledge is a social phenomenon, it is not always the case that well-directed criticism will have impact. Indeed, often it has very little at all, for personal stakes and social commitments often have as much to do with the acceptance of criticism as does its validity.

There are at least three broad categories of criticism, all construed here to be constructive. One involves that which refutes a piece of research or purported finding, or relegates it to a rather trivial position within the hierarchy of knowledge, and which, in the act of criticism, offers nothing to replace it. A second type is similar, differing in that the critical act entails either a replacement set of statements for similar facts, or statements about how a new test, model, or theory might be initiated. A third type of criticism is devaluation. Here a result or finding is not thrown out wholesale; rather some aspects are excluded from criticism or judged to be of particularly good quality and other aspects are labeled — for specific reasons — as inferior or not worthy of future attention.

Except for what we have called primitive criticism, the destructive act of refutation is the contrary of its apparent meaning: *destruction is construction.* Whenever skepticism and criticism have the potential to improve science as product, they are constructive. We do not attempt to deny that skepticism and criticism are not socioepistemological matters — they surely are. Rather, we urge scientists to strip these concepts, for purposes of use in science, of their palpable negative connotations of harmfulness.

These definitions are somewhat arbitrary but convenient within our defined purpose; they also seem to fit well within the spirit of healthy science. It is tempting to think of skepticism in the context of particular doctrines that relate to the denial of certain religious principles, the belief that any true knowledge is impossible, that all knowledge is uncertain, or that no truths can be established on purely philosophical grounds. These are — for our purposes — spurious connections. They are of philosophical or religious interest, but of little concern here.

Skepticism and Criticism Among Social Scientists

Kenneth Boulding has remarked that "knowledge grows by accretion, but we gain truth by pruning the tree of knowledge" (Kerman 1974:39). Merton, a long-time student of the sociology of science, pinpointed "organized skepticism" as that element of the scientific ethos which is "both a methodological and an institutional mandate" (Merton 1973:277). He believed that organized skepticism is responsible for the relative paucity of fraud in science. He was unconvinced that scientists have an inherently higher sense of moral integrity than anyone else. Rather, "the activities of scientists are subject to rigorious policing to a degree perhaps unparalleled in any other field of activity" (Merton 1973:275).

The organized skepticism of the scientist, according to Merton, is sufficiently powerful that it functions not only as a methodological canon, but tends to spill over into the critical questioning of values generally. The scientist learns to ignore the distinction between the sacred and the profane. He questions authority, routine, vested procedures, the crystalized and ritualized facts and data all around us:

> *Most institutions demand unqualified faith; but the institution of science makes skepticism a virtue. Every institution involves, in this sense, a sacred area that is resistant to profane examination in terms of scientific observation and logic. The institution of science also involves emotional adherence to certain values. But whether it be the sacred sphere of political convictions or religious faith or economic rights, the scientific investigator does not conduct himself in the prescribed uncritical and ritualistic fashion. He does not preserve the cleavage between the sacred and the profane, between that which requires uncritical respect and that which can be objectively analyzed (Merton 1973:265).*

Whether Merton's characterization is true generally, or is primarily a depiction of those he has known in disciplinary context, is a moot point. Perhaps there is considerable variation among scientists, both within and between disciplines.

Karl Popper has probably argued the strongest and most persuasive case for skepticism and criticism in science. He claims that two of the most important evolved traits of human language are the descriptive and the argumentative functions. Through the descriptive function, truth arises in the correspondence between facts and language, while in the argumentative faculty, correction is introduced into the system via

criticism. For Popper, the critical and the rational are inseparable, almost equatable. Indeed, the argumentative function has had "the most decisive influence on the human ability to think rationally" (Popper 1974:237). Error elimination results, then, from systematic rational criticism (Popper 1974:121). Rationality, argumentation, consciousness, and error elimination are human traits epitomized in the scientist:

> *The main difference between Einstein and an amoeba is that Einstein* consciously seeks for error elimination. *He tried to kill his theories: he is consciously critical of his theories which, for this reason, he tries to* formulate *sharply rather than vaguely. But the amoeba cannot be critical* vis-a-vis *its expectations or hypotheses; it cannot be critical because it cannot* face *its hypotheses; they are part of it. Only objective knowledge is criticizable: subjective knowledge becomes criticizable only when it becomes objective (Popper 1974:24-25).*

Criticism, the use of "negative arguments," is the scientist's principal instrument in the further growth of knowledge. In a sense, criticism shows the willingness of scientists to take discrepancies between theory and observation seriously, that critical step which Kuhn identifies with progress in science (Kuhn 1962). To Popper it is more; the argumentative function makes science "one of the greatest creations of the human mind," precisely because "our explanatory myths become open to conscious and consistent criticisms. . . ." (Popper 1974:84). Popper sees the argumentative function of sufficient importance to claim that the primary difference between a scientific theory and a work of fiction lies not in truth, but rather in that each is embedded in a different critical tradition. It is not that criticism is foreign to fictional portrayals, it is just not so fundamental to meaning and motif. It cannot be, if only because growth in scientific and fictional circles means such fundamentally different things. One builds on and prunes the past, the other does not.

Though the creative act of the scientist requires that passions be given their place and their play, they are not to be confused with the rational act of validation and the need to subject intuition and creative insight to critical examination. Hume's famous assertion that "reason is, and ought only to be the slave of the passions, and can never pretend to any other office than to serve and obey them" is inappropriate to scientists.

We noted that the commonplace connotations of skepticism and criticism are those of destruction and negation and that they have, as a result, socially reproachable overtones (Russell 1928:13). But we also noted that in healthy science, social meaning is not the same as scientific meaning. To the properly trained scientist, the negative is the positive, and the irrational, the uncritical is the retrogressive, the status quo. To emphasize or cherish what one has already learned or believes to be the case, rather than to subject knowledge to continuous skepticism and critical examination, is, to draw a parallel from the area of political economy, to be counterrevolutionary (Harvey 1972b). Science is the past, but even more so it is the present and the future.

Scientists behave in ways counterrevolutionary because they are social animals, because they are products of their past, and because they have vested interests in what they and others have done which transcend the rational. It is all too easy to be counterrevolutionary: "people are not naturally sceptics, wondering if a single one of their intellectual habits can be reasonably preserved; they are dogmatists angrily confident of maintaining them all" (Santayana 1923:11-12). Sometimes it takes an explosive social issue, such as the alleged relationship between race and intelligence, to force scientists out of the mold of accepting that which is paraded before them (Wade

1976). But scientists must be critical, and consistently so, nay, vigilant revolutionaries, because like questions of social justice, the future can be better than what we know and possess. Popper is a revolutionary:

> *In my view, all that can possibly be 'positive' in our scientific knowledge is positive only in so far as certain theories are, at a certain moment of time, preferred to others in the light of our critical discussion which consists of attempted refutations, including empirical tests. Thus even what may be called "positive" is so only with respect to negative methods (Popper 1974:20).*

If Popper's case and our extension of it seem too strong, the arguments lose their sharper and harsher edges when one is reminded of the nature of knowledge itself.

> *The totality of our so-called knowledge or beliefs, from the most casual matters of geography and history to the profoundest laws of atomic physics or even of pure mathematics and logic, is a man-made fabric which impinges on experience only along the edges. Or, to change the figure, total science is like a field of force whose boundary conditions are experience. The total field is so underdetermined by its boundary conditions, experience, that there is much latitude of choice as to what statements to re-evaluate in the light of any single contrary experience. (Quine 1966:414-15).*

If one accepts Quine's picture of knowledge and how we relate to it as scientists, then in the context of arguing for the necessity of skepticism, one is lead to conclude that since we do no more than sit on a thin boundary of experience, and it is from such a vantage point that adjustments are made in the entire field, it is particularly important that we examine known experiential statements with the utmost care. Were science a set of better connected statements than it is, or were our experience more than a mere dot on a huge sphere, the necessity of skepticism might not be so great. The type and extent of needed corrections would be more apparent.

The problem introduced by Quine is compounded by what Whitehead has referred to as "the dogmatic fallacy":

> *The error consists in the persuasion that we are capable of producing notions which are adequately defined in respect to the complexity of relationship required for their illustration in the real world. The notions employed in every systematic topic require enlightenment from the perspective of every standpoint. They must be criticized from the standpoint of their own internal consistency within that topic, and from the standpoint of other topics of analogous generality, and from the standpoint of so-called philosophic topics with a wider range (Whitehead 1967:144).*

Dogmatism, of course, is a coin with two sides, and it is therefore well to remember Santayana's observation that skepticism itself can be a dogma if it becomes academicism (Santayana 1923:8-9).

Norms and Counter-Norms

If philosophers and scientists accept the central role of skepticism in the scientific enterprise, yet there is a myriad of reasons why it is often underplayed, then we

should ask how it is that norm and practice fit together. Polanyi, Nash, and Merton shed light on this question by noting that the scientist can accept one method while actually practicing another (Polanyi 1958:169; Nash 1963:322; Merton 1976:57-58).

Attitudes and values which the scientist advocates are listed in the left column of Table 1, practices of the scientist are on the right. These pairs of seemingly polar antitheses have been referred to as dominant and subsidiary norms, or as norms and counter-norms (Merton 1976:58). Counter-norms are simply "understood"; in Polanyi's terms they are "tacit knowledge" that remain as apparently unreconciled. According to Merton, it is through the structures of norms and counter-norms that various functions of a scientist's role can be effectively met. The scientist is faced daily with ambivalences not unlike those faced by all humans; if they seem to persist as irreconcilables, as apparent contradictions, it is because such ambivalences are built into the very nature of role definitions.

But Nash argues that the matter is more complicated. There is interaction between the polar opposites:

> *How do we maintain such ambivalent mixtures of faith and scepticism? That question arises only from the misconception that makes antitheses of inseparable polarities. Faith and scepticism are not more than opposite faces of the same coin. Nowhere outside the asylum is there a wholly pure faith or a wholly pure scepticism. Scepticism of one authority is the measure of faith in another, and conversely. Consider Copernicus. He is sceptical of the everywhere-accepted Ptolemaic conception. The root of his scepticism? Faith, of course – faith in the Pythagorean conception of a cosmos mathematically harmonious (Nash 1963:323-34).*

Nash may oversimplify the matter in missing the social fact that not all coins in fact have two sides. Primitive criticism, where one condemns a finding or piece of work for no apparent or articulated reason, may have no "faith" basis whatsoever in an alternative belief, theory, or fact. Physicists and other natural scientists, by virtue of having more articulated theories and more clearly institutionalized criteria for establishing the merits of research than social scientists, may not be particularly prone to primitive criticism. But there is no doubt that primitive criticism accomplishes little in the social sciences.

Whatever the differences between the hard and the soft sciences, Nash does seem to resolve a paradox. Like Merton he reminds us of the need for balance between a "passion for knowledge" on the one hand and "organized skepticism" on the other (Merton 1976:62). It might be argued that Table 1 is a fully connected matrix; overemphasis on any one of the norms or counter-norms produces, if not unhealthy science, science which moves forward at less than maximum efficiency.

TABLE 1 NORMS AND COUNTER-NORMS

NORMS	COUNTER-NORMS
Empiricism (and facts)	Speculation (and hypotheses)
Skepticism (and detachment)	Faith (and commitment)
Logic	Imagination
Order	Chance

Source: Merton 1976:57.

Although the role of skepticism and criticism in the scientific arena has been most thoroughly elaborated by philosophers and scientists outside geography, some geographers have commented on the matter. Zelinsky, for example, has noted that "one of the small pleasures of the scientific life is that a certain degree of vituperation — directed solely against a colleague's professional follies, of course — is encouraged" (Zelinsky 1975:133). Nearly forty years earlier Leighly began a major methodological critique with the statement that "practitioners of scholarly disciplines have always been accorded the privilege of criticising the work of their fellows" (Leighly 1937:125). Other geographers, such as Hartshorne, Whitaker (1954), and James, have expressed the need for criticism. In his AAG presidential address, James saw the "persistence of error in geography" as a barrier to progress in the discipline. Although James favored a "continuing process of critical review" of scholarly output, he did not see the connection between order and skepticism. Rather, he believed that the problem resided in the "persistent failure of too many geographers to read what other geographers, past and present, have written" (James 1967:21).

In this century few geographers have been subjected to as much criticism as Hartshorne, largely because of *The Nature of Geography* (Hartshorne 1939; 1948; 1954; 1955; 1959). Hartshorne reacted to his critics in several ways. Not only did he methodically attempt to answer them, but he also wrote on the need for criticism, saying at one point that "anyone who says that he is not interested in critical analysis of the logic of his writing is saying that it is of no concern to him whether his conclusions are sound, dubious, or completely erroneous" (Hartshorne 1948:117).

Hartshorne's views can and should be applied generally within the discipline:

> *It is both appropriate and desirable that any methodological writing worthy of publication should be subjected to critical evaluation in terms of its basic assumptions, the reliability of its evidence, the soundness of its logical reasoning, and extensive comparison with the findings of other students of the same problem (Hartshorne 1948:117).*

While fully recognizing the value of criticism, Hartshorne nevertheless felt strongly that critics ought to observe certain rules (Hartshorne 1955:207-208). In good part, this seems to have been a reaction to what he saw as extremely sloppy and unprofessional criticism of *The Nature of Geography* by Schaefer (Schaefer 1953; Hartshorne 1954; 1955).

Beyond these few examples, geographers have not given much attention to skepticism and criticism, and none seems to have seen their inevitable linkage with the principle of order. It might seem that skepticism and constant criticism would fare particularly well with the positivist philosophy widely adopted by geographers in the 1960s. After all, positivism, at least in practice if not in theory, involves making judgments about the character of methodology rather than the discovery of truth (MacKenzie 1977:37-38). But skepticism has not been a hallmark of positivist thinking in geography. Indeed, of all those geographers cited for their pronouncements on skepticism and criticism, not a single one is a positivist. Skepticism has been absent from much recent work at least in part because of a confusion of abstract theorems with actual behavior (Georgescu-Roegen 1971:82). Having postulated some model of reality, the model is read *into* reality and order proclaimed. However, the lack of attention to skepticism is not easily explained. We must examine in detail why attention to order has become so significant for many geographers as to become their definition of science.

2

In Pursuit of Order

In the period from about 1955 to the present, when the discipline of geography experienced a "quantitative revolution" and witnessed the ascendency of logical positivism and empiricism as philosophies of research, order as a principle was elaborated into a dogma more pervasive and profound than was ever held by those who practiced an environmentalism that proceeded from a similar principle (Burton 1963; Rushton *et al.* 1971:49). We identify eight indicators of this claim; there may well be more. We also discuss a major consequence of the emphasis on order for the practice of research, the self-fulfilling prophecy.

Acceptance of the Nomothetic

One indicator was a sharp turn toward and acceptance of the nomothetic point of view (Kohn 1970; Sack 1972). Indeed, it was the fundamental "dissatisfaction with idiographic geography [which was] at the root of the quantitative revolution" (Burton 1963:156). The severity of the simultaneous rejection of old and acceptance of new was categorically commanded by Bunge, among others: "Only by the complete rejection of uniqueness can geography resolve its contradictions" (W. Bunge 1962:13), and "science is the deadly enemy of uniqueness. As the masterful Schaefer taught us, generality is science's weapon in our unending reduction of uniqueness" (W. Bunge 1966:376). Change, of course, is not necessarily synonymous with progress:

> *It is as if geography is re-emerging after the lapse into idiography which followed the retreat from environmental determinism. The quantitative revolution is taking us back much closer to environmental determinism. It is surely not coincidental that the quantitative revolution is contemporaneous with the appearance of neo-determinism in geography (Burton 1963:152).*

To be sure, the nomothetic perspective was not all-pervasive in impact, and there has been a much greater emphasis on probabilistic modelling since Burton's 1963 statement. Yet, the renewed and perhaps accentuated dogma of the ordering principle became widespread among geographers. There is little doubt today about the relative insignificance given the idiographic point of view by younger generations. For most geographers, Hartshorne's work is an anachronism. Schaefer's words, which confuse

science with the spatial, continue to ring loud and clear. "Science is not so much interested in individual facts as in the patterns they exhibit. In geography the major pattern-producing variables are, of course, spatial" (Schaefer 1953:228). Though idiographic studies remain important in research and in published results, and probably always will, the nomothetic-idiographic argument engenders little interest now (Berry 1960; Robinson 1961; Spate 1960a, 1960b).

The Language of Discourse

Language reflects as well as shapes one's reality. Most geographers today, even those whose research has an idiographic component, talk about models, theories, and hypothesis testing. All imply order: if not discovered, then searched for; if imperfectly known for some domain of inquiry, then an area in need of further research.

It is perhaps curious that despite the talk of modelling and theory building, recent positivist statements must point to probability theory as an example of a well-formed theory. Only reluctantly do geographers turn to their own discipline for possible candidates for law and theory status (Amedeo and Golledge 1975), and for good reason:

> *There are no general laws in social science that are consistent over time and independent of the context in which they are imbedded. The search for law-like generalizations of cause and effect relationships is an illusion. Any particular patterning of events will not remain stable for very long. . . (Rein, quoted by King 1976:304).*

In a sense, of course, it does not matter that well-constructed theories and laws are more a matter of armchair philosophizing and wishful thinking than demonstrated fact. Even though "geographic" theories do not and probably never will meet the criteria established by certain philosophers of physical science (from whom the ideal in the form of the hypothetico-deductive explanatory model is drawn), they are theories of a sort in being so labeled. Convention is meaning. Theories, models, and hypotheses, labeled in our writings and described in our conversations, are the second indicator of the dogma of order.

Decline in Attention to the Regional Concept

A third change which has strengthened the universalizing search for order involves the change in status of the regional concept (Ackerman 1961; Thoman 1965; Taaffe 1970). On the eve of logical positivism's baptism, which Amedeo and Golledge designate as 1955, the regional concept occupied probably *the* pivotal conceptual position in geography (Amedeo and Golledge 1975:iv). This was indicated in an inventory and prospect for the discipline at the time (James and Jones 1954:19-58). By 1965 one could say that "the regional method . . . remains the means for much geographical work but is no longer its end," and "that much geography is still regional, but no longer that geography is about the region." (Wrigley 1965:13).

Although regional studies, like the idiographic (there is considerable overlap), will continue to occupy a prominent place in geographic inquiry, the role of the former is less significant than it was prior to 1955. With new and expanded concern for order it could not be otherwise. Laws and theories, in principle, are constructs for all places and times. To speak of regional laws or regional theories — no matter the validity of such middle

level abstractions — suggests a fundamental contradiction. By definition, a regional law means that it is less than universal, while a universal law, by definition, must apply to all regions. Although universality in these terms has a strong idealistic ring to it, even among those imbued with positivistic thinking, it remains a metaphysical norm. For many geographers, like many social scientists, the model for science is the positivist image of the hard sciences (Gregory 1978:25-48; King 1976:300; Lessnoff 1974:55, Meehan 1968:v).

The Tie to General Systems Theory

Another index of the search for order, perhaps one of the more extreme attempts by geographers and other scientists in this regard, has been the effort to link up with general systems theory (Batty 1974; Chapman 1977; Chisholm 1967; Chorley and Haggett 1967; Langton 1972; Mabogunje 1970; Marchand 1972; Miller 1965; Stoddart 1965; Symanski 1968; von Bertalanffy 1956; Warntz 1973; Wilbanks and Symanski 1968; Woldenberg and Berry 1967; Young 1965). General systems theory is a viewpoint that attempts to formulate and deduce principles applicable to all types of systems — physical, biological, and social. As a body of knowledge, general systems theory is a set of descriptive, explanatory, and predictive statements and concepts culled from and contributed to by all branches of science (von Bertalanffy 1956; Miller 1965). Philosophically, systems theory places attention on complex, whole systems; urges the use of a mathematical language — where possible — and a common vocabulary; and advocates close interdisciplinary ties to facilitate the transfer of ideas and models. In short, the mandate is to search for order beyond that normally sought within the narrow confines of a single discipline.

Geographers have made a number of attempts to apply this philosophy. They have used the jargon of general systems theory: open and closed systems; negative and positive feedback; information, entropy, and steady state, to mention but a few terms. Information theory, game theory, entropy maximizing models, and the law of allometric growth are models associated with a general systems theoretic perspective that geographers have applied in spatial and ecological contexts. In some instances, as with central place theory and the rank-size rule, there have been efforts to subsume theoretical and empirical findings under general systems theory, or argue that meaning is inextricably linked to a systems theoretic concept (Berry 1967: 76-79; Bromley 1978; Gould 1963; Maruyama 1968).

While all these efforts can be seen as attempts to reap the benefits of different or more developed models and theories found outside geography, a common underlying motive has been the belief that the findings of geographers are examples of more general kinds of order. Efforts in this direction by geographers and others may be admirable, even if — with a few major exceptions — the attainments have been modest or the alleged higher-order generalities belabor the obvious.

Case Study Implications

Geography, like other social sciences, has institutionalized the practice of implying that one or two case studies constitutes a sufficient test of a theory, again evidence for the dogma of order. Certainly no respectable theory in the social sciences, even those which are bracketed in the sense of not being cross-culturally applicable, rests or falls on the basis of one or two tests. At a philosophical level the need for multiple validations

is apparent, the argument resting on the straightforward idea that a theory is a set of interrelated lawlike statements with empirical referents that are repeatedly confirmed (Rudner 1966:10).

Geographers with positivist inclinations who have addressed the matter of theories and lawful statements have exhibited difficulty in giving examples of well-confirmed, formalized theories (Amedeo and Golledge 1975:41-49; Harvey 1969). Their best examples are largely meaningless: for example, a probability theory without empirical content. When an "empirical theory" is singled out for exemplary status, as did Amedeo and Golledge with Hudson's theory of rural settlement, it all seems a bit odd. Hudson's "theory," as it were, has not been followed up outside its quite specific geographic context. Indeed, it is clear to any student of history that there are all kinds of places it would not work. Its value seems to rest on the fact that it is built around a set of interrelated ecological generalizations from biology (which do not have lawful status in that discipline) from which geographically relevant hypotheses can be deduced and formally tested. A single confirmation has not deterred geographers from allowing the "theory" to be proudly singled out (Amedeo and Golledge 1975:63; Hudson 1969a, 1969b).

The general issue here is not the nature or status of Hudson's "theory," nor the fact that positivist geographers are not unaware of these issues, nor even that many "theoreticians" may not willfully intend to provide anything more than a first test of theory. Rather, the point is that case studies — in the study of central places, diffusion, transportation, and various kinds of urban-economic activity — have implied that they are *representative* examples of the phenomenal universe of concern. One might argue that there is good reason for this kind of sinning by implication. Were case studies to be seen as possibly unique in significant details, then it might also have to be admitted that geography has not moved all that far from its old idiographic position. One might have to admit that a good bit of the difference between the old and the new is jargon, use of mathematics, and a presumed "rigor."

In one sense the "case study problem" bears a resemblance to what Fischer has referred to as the "fallacy of the lonely fact." He illustrates the fallacy with a story, perhaps apocryphal, of a scientist who published an unlikely generalization about rat behavior:

> *An incredulous colleague came to his laboratory and politely asked to see the records of the experiments on which the generalization was based. "Here they are," said the scientist, dragging a notebook from a pile of papers on his desk. And pointing to a cage in the corner, he added, "There's the rat" (Fischer 1970:109).*

Instrumentalism versus Realism

That instrumentalism has played no small role in the geography of the last couple of decades is additional evidence of dogmatic ordering (Guelke 1971: 44, 53; Olsson 1975; Rushton *et al.* 1971:107). To the realist it is important that statements of order truthfully explain aspects of reality; to the instrumentalist the significant issue is the match between reality and prediction or control. If a predicted pattern bears a reasonably close relationship to reality, then the model (or theory) is judged to be valid; if it does not, then the search begins anew. For the instrumentalist, it may be of some ultimate

import that the statements (assumptions) from which the predictions (testable hypotheses) follow be realistic, but such concern is in large part secondary (Rescher 1970:130). Nagel states the instrumentalist's position somewhat differently, but the message is the same:

> *The central claim of the instrumentalist view is that a theory is neither a summary description nor a general statement of relations between observable data. On the contrary, a theory is held to be a rule or principle for analyzing and symbolically representing certain materials of gross experience, and at the same time an instrument is a technique for inferring observation statements from other such statements (Nagel 1966:427).*

Instrumentalism fits nicely with the positivist definition of science adopted by many human geographers (and their critics). Method is one's master (Gregory 1978:42). But when a methodological definition of knowledge dominates research, theory as a source of discrimination of knowledge is secondary or irrelevant. There is no theoretical basis for preferring or rejecting any specific order statement, for we have not asked any questions which would lead us to know whether our statement accounts, in a *process* sense, for a slice of reality. Here, of course, is the central content of realism as an epistemological position. With instrumentalism, aesthetic principles, such as the "Principle of Simplicity," substitute for theory (Harré 1965; M. Bunge 1963; A. S. Skinner 1979).

Westkott (1977) has argued that under a positivist/instrumentalist image of science, with its emphasis upon usefulness rather than truthfulness, repudiation of the unobservable, and stress on control and measurement, theory is ultimately subsumed into the logic of the scientific method. She writes:

> *A science that is committed only to the recording of empirical facts has no basis upon which to rationally judge these facts, to assess them according to some idea of historical truth or* alternative interpretations (Westkott 1977:70-71; our emphasis).

The geographer Guelke is more pointed in his criticism:

> *One looks in vain for objective criteria for which a good model can be differentiated from a poor one. The new geographers claim that their methods are more scientific, without supporting this claim (Guelke 1971:48-49).*

If one problem lies in believing that method is theory or that the latter can be reduced to predictive statements, another is that instrumentalism does not encourage skepticism. Indeed, instrumentalists seem to believe that the order which emerges from the application of a particular method is inviolable.

If realism, rather than positivism/instrumentalism, were the model of science chosen by human geographers, then this specific stimulation to order at the expense of skepticism would not be so significant. For the realist, adequate explanations require the discovery of both regular relations between phenomena *and* the processes which link them (Harré 1972).

A particulary unfortunate consequence of the emergence and dominance of instrumentalism has been the tendency of some researchers to mix instrumentalism and realism in the construction of their theories. Some geographers infer curious things

when they hear the term instrumentalism. One group understands that there is no interest in truth or falsity. Rather, the question is one of theory scope: can a particular set of empirical observations be satisfactorily accounted for by means of this or that method of curve-fitting? Others equate and mix instrumentalism and realism. They believe that the function of instrumental models has something to do with truth and falsity, when in fact this is not the case (Toulmin 1960: 112-113, Nagel 1966:431).

The Meaning of Hypothesis Testing

A seventh indication of an expanded concern with order concerns the practice of hypothesis testing. A scientist should be as prepared to reject a hypotheses as he is to accept one, and, of course, some geographers do. But Abler, Adams, and Gould make a significant point in noting that:

> *because we have no* Journal of Negative Results, *scientists usually report to each other only those hypotheses they have been able to confirm: we must realize that we are looking at a very biased sample of all the hypotheses which have been formulated and tested (Abler, Adams, and Gould 1971:32)*

Geographers, like other scientists, learn early that unless a rejected hypothesis is of some significance in that it is related directly to an established finding of import, it is best stated in its contrary form. (A telling, slightly cynical remark among scientists is that accepted hypotheses are often inductively-derived conclusions from statistical fishing expeditions which have been written at the beginning of a paper to satisfy established journal conventions.)

Positivist geographers are, in the main, not very upset about the problem of negative results. Because their models quite frequently include a *ceteris paribus* clause, there is little worry about refutation. If a model does not work, if one does not get the expected relationship, then it can be claimed that the problem lies somewhere in the quagmire called "other things being equal" (Gregory 1978: 66). Herein lies a plenitude of *ad hoc* hypotheses and a large and undefined set of variables that deny the possibility of falsification.

Acceptance of a hypothesis is of particular concern when alleged relationships are "new." In science, a heavy premium is placed on priority of discovery, and it is one of the principal means of assigning rewards to individuals (Merton 1973: 279-322). But this criterion, when closely entwined with the principle of order, can lead to serious problems:

> *The great cultural emphasis upon recognition for original discovery can lead by gradations from . . . rare practices of outright fraud to more frequent practices just beyond the edge of acceptability, sometimes without the scientist's being aware that he has exceeded allowable limits. Scientists may find themselves reporting only successful experiments or results, so-called, and neglecting to report 'failures' (Merton 1973:311).*

Even if the issue is not that of genuine originality, budding scientists learn through a seldom written but nevertheless pervasive acculturation process that the more positive

the results — the better the correlation — the more successful one ought to feel. That is, success is not only tied to precedent and the discovery of order, but also to uncovering *more* order than others have found. Or to put it differently, the question is not that of the explained variance per se, it is a matter of portraying unambiguous results. Stephen Jay Gould, a geologist and biologist with an interest in scientific methodology, contends that:

> *I suspect that unconscious or dimly perceived finagling, doctoring, and massaging are rampant, endemic, and unavoidable in a profession that awards status and power for clean and unambiguous discovery. This is the middle ground of unappreciated bias and more conscious manipulation in the interest of a 'truth' passionately held but inadequately supported (S. J. Gould 1978:504).*

Various other strategems are open to the scientist committed to finding order. Besides the suppression of negative instances, there is the selective reporting of data. Some data fit a hypothesis better than other data, and some fit the hypothesis not at all. Selective pruning, easily rationalized by quality, area, or temporal criteria, makes it relatively easy for one to make the desired case (S. J. Gould 1978). A related strategem is to purposely choose a case "a priori" which has a high probability of leading to hypothesis acceptance (Fearnside and Holther 1959:15). It is an interesting historical fact that a number of the early tests of central place theory were undertaken precisely where the fit between predicted patterns and reality might have been expected to be closest. To argue that this was done to better "understand" either the place or the theory is somewhat nonsensical. Central place theory contains a number of assumptions, no large combination of which can be held relatively "constant" in any given real place. Furthermore, then as now, it is difficult to know how to cope either mathematically, or analytically without mathematics, with the meaning of the difference between model pattern and reality.

It is enough to be moralistic about fudging or outright fraud, and equally easy to see a major component of the problem in the ordering principle, but it needs to be remembered that insight and creativity may be occasional bedfellows with apparently reprehensible practices. Some of the greatest scientific discoveries by some of history's best minds have involved finagling, perhaps more. Newton had to fudge unreasonably to support crucial statements he could not prove, and biologists now know that Mendel's F_2 ratios are too good to be believed. But as Stephen Jay Gould reminds us, "we measure greatness not by 'honesty,' but by insight. After all, Newton and Mendel were right" (S. J. Gould 1978:504). None of this, of course, is to argue that fudging or fraud should be encouraged. They must be condemned on principle (Wade 1976).

The Display of Accumulated Knowledge

A final sense in which one sees a premium placed on order as a guiding concept is in attempts to display "accumulated knowledge" in textbooks, monographs, and other learned outlets. The often-debated systematic-regional dichotomy in geography has a certain value in this context. 'Systematic' more clearly implies an interest in the general or universal case. It is not surprising, therefore, that systematic approaches to writing textbooks are today taken for granted; that regional books show a greater concern for using locational analysis; and that other text efforts which express a renewed interest in the regional concept, but whose authors proceed from a nomothetic premise, cast their

place-specific material within a larger, systematic framework (Berry *et al.* 1976). The concept of a case study is a way of drawing attention to the fact that either one has found some kind of universal order, which is being demonstrated, or that there is reason to believe that the quest is of the right sort and will be accomplished when enough case studies are assembled.

The Self-Fulfilling Prophecy

We cannot see contrary evidence, disorder or lack of pattern, if we are not prepared to do so, particularly if attitudes and practices reinforce a central, fundamental dogma. The principle of order is akin to a language. It limits one's world view. As Wittgenstein noted in the *Tractatus*, "That the world is *my* world shows itself in the fact that the limits of *my* language (of the only language I can understand) mean the limits of *my* world" (Wittgenstein 1923:5.62). The scientist, the logician, and the philosopher all face the same problem:

> *The logician's perennial temptation [is to] make the portrait neat and perhaps the sitter will become neat. Usually there is more to be learnt from a study of the disarray than is gained by intentionally disregarding it (Scriven, quoted in Kaplan 1954:26-27).*

In a similar vein but with attention to social and cultural factors, Whitehead noted:

> *The chief danger to philosophy is narrowness in the selection of evidence. This narrowness arises from the idiosyncrasies and timidities of particular schools of thought, of particular epochs in the history of civilization. The evidence relied upon is arbitrarily biased by the temperaments of individuals, by the provincialities of groups, and by the limitations of schemes of thought (Whitehead 1950:369).*

As suggested in these quotations, a major part of the problem with strong preconceptions, particularly when not subject to regular scrutiny or a countervailing mechanism, is the real danger that they will lead to still another self-fulfilling prophecy. The young, impressionistic scientist is imbued with the necessity to find pattern, learns that success is tied to such a venture, and then proceeds to "discover" what was expected all along. It is not simply that one will or will not see order as some function of cultural lenses or as related to the distance (metaphorically speaking) that one stands from the object or events of concern. The learning and the biasing path is more direct and less philosophical: the scientist quickly learns that proper observational or analytical distance is the one which is successful, and that other distances are not so much idiosyncratic as "unscientific."

The self-fulfilling prophecy is related to how we conceive order, as an inherent property of Nature or social relations or as a perceptual and linguistic phenomenon.* Those who adhere to the first position believe that order is out *there*; it is the investigator's task to find, describe, and explain. But even if this is the position taken by a

*Ways of conceiving order are more complex than discussed here. For example, Whitehead identifies four principal doctrines concerning the Laws of Nature: the doctrine of Law as imminent, the doctrine of Law as imposed, the doctrine of Law as observed order of succession, and Law as conventional interpretation (Whitehead 1967:11-18).

scientist, "no one either assumes, or has discovered, or expects to discover an unlimited degree of uniformity in an unlimited number of respects" (Toulmin 1960:152). The position that order is imposed has been the most popular of the two. It assumes the least (and therefore heeds the widely accepted scientific Principle of Simplicity); and it makes good sense in light of social science theories. Not surprisingly, it finds most favor among social science philosophers. For example, Kaplan's position is straightforward:

> *It must . . . be emphasized that the order of a set of objects is something which we impose on them. We take them in a certain order; the order is not given by or found in the objects themselves (Kaplan 1964:180-81).*

In general, the greater the demonstrated interconnections among theories, the greater the number of empirically meaningful statements within these, and the greater the predictive power of the theories, the more reason one has to believe that imposed and inherent order coincide. Since social science (and geographic) theories are structurally weak, poorly interconnected, and generally have poor predictive powers, one can perhaps argue only tentatively that order is inherent and pervasive in the social world. But while recognizing that this means there are problems, this is not the same as the argument that "everything in science, and mathematics too, seems to be tentative and makeshift — perhaps just barely good enough for the purpose at hand" (Zelinsky 1975:131-32). Such statements possess polemical value, but they are not accurate representations of science as a process or product. Mitroff (1974) and others who picture science as little more than an aggregate of subjective opinions have tremendous *chutzpah*, to put it lightly. They give "a storybook version of scientific inquiry" (Merton 1968:16).

The existence of different order premises opens the possibility that distinctions among them will be blurred or erased, with unhealthy consequences. If the working premise is that of inherent order in the world, then reality is the foremost guide to approach, as well as the constant referent and reminder as to how linguistic and artificial systems of order ought to appear. If, on the other hand, one begins with a principle of imposed order, as geographers have increasingly done in recent decades, then other concerns are paramount: tractability, simplicity, predictability, and mathematical elegance as ends in themselves (King 1976:294, 301-302). Only secondarily is the concern reality. Instrumentalism is the guiding philosophy in this respect (Guelke 1971; Entrikin 1977).

The instrumentalist must constantly guard against equivocation. Some of the statements used to create order are clearly at odds with reality. Assumptions of *Homo economicus* and an undifferentiated plane are two common to geography. However, one may believe that when there is only a small difference between instrumental models and reality, the consequential statements represent an order inherent in reality. One kind of order — imposed — is substituted or equated with another — inherent — and no distinction thereafter remains between two critically different forms of the assumption. In short, the researcher begins with the premise that order is a condition of his perceptual stance and ordering statements and ends by claiming that an artificial ordering device is a true picture of a real world order (Georgescu-Roegen 1971).

Another sense in which a universalizing conception of order can cause serious problems, with self-fulfilling overtones, is in over-extending the singular case. One who begins from a particularistic principle of order is disinclined to generalize beyond the specific time-space context of inquiry. Those who use such studies are reminded by

context and mode of presentation that the findings are place and time bounded. But when the dominant premise in a discipline is one of universalizing order, there is a real possibility — often realized — that one will, by design or neglect, extend the singular finding to universality when not warranted. Even if the researcher is not culpable, readers who begin from a similar assumption may make the mistake.

The self-fulfilling prophecy is a matter initially philosophical, ultimately methodological. Mario Bunge has pointed to the connection between the self-fulfilling prophecy and the linear regression model, one of the most common statistical tools in the geographer's kitbag:

> *The computation of correlation coefficients and fitting of regression lines should not be taken for a law-finding method, as is so often the case in the behavioral sciences. When a linear regression model is assumed and the parameters are computed from the data, the central law that is supposed to run through the "noisy" (scattered) information is not found but assumed beforehand. No amount of statistical data processing produces new hypotheses by itself, let alone laws . . . (M. Bunge 1967:316).*

The self-fulfilling prophecy operates at several levels: problem selection and definition, data selection and gathering, tool choice and data interpretation, and final dissemination of the results. Of the various crossroads where a scientist can circle back to homebase and tell stories of a destination not, in fact, reached, one concerning data gathering creates a problem which does not exist for the physical scientist. This is because, as Merton has noted with examples, "predictions of the return of Halley's comet do not influence its orbit. But the rumored insolvency of Millingville's bank did affect the actual outcome. The prophecy of collapse led to its own fulfillment" (Merton 1949:181).

Influencing the outcome of a social event may be one of the most significant aspects of self-fulfilling prophecies in a social scientific context, especially when coupled with the previously introduced issue of fudging or fraudulent behavior. Stephen Jay Gould has described how the nineteenth century Philadelphia physician, Samuel George Morton, possessor of the world's largest collection of human skulls in the nineteenth century, measured their capacity and showed by means of a number of dubious practices that in terms of innate superiority Whites were better than Indians, and Blacks were at the bottom (S.J. Gould 1978). Among the self-fulfilling practices which gave Morton the case he wanted were the deletion of large subsamples which did not accord with his expectations, the use of shifting criteria when calculating cranial means, the unwillingness to consider alternative hypotheses such as the obvious correlation between cranial capacity and body size, and errors of miscalculation and recording of results — which happened to be in his favor. What is apparently curious, yet perhaps understandable is that there is no indication of conscious fraud on Morton's part:

> *Morton made no attempt to cover his tracks . . . he explained everything he did, and published all his raw data. All I discern is an a priori conviction of racial ranking so powerful that it directed his tabulations along preestablished lines. Yet Morton was widely hailed as the objectivist of his age, the man who would rescue American science from the mire of unsupported speculation (S.J. Gould 1978:509).*

How many geographers, it might be asked, have found out what they knew they were going to "discover" beforehand about the functioning of Third World ecosystems or First World central place systems, not because of insight, good intuition, or good theory, but rather because of the leading questions asked, the self-fulfilling impact on interviewees, or the methodologies employed? No one really knows the answer. No one knows because methodologies are so poorly specified, and never completely, even in the best of descriptions. No one knows because information is lacking on the biases of those who have done fieldwork or sustained research. We simply do not know in any detail how order and skepticism as principles fit into the total metaphysical viewpoint of scientists (Hammond 1964). Asking such questions probes the psychology and the sociology of individual scientists, and this the great majority do not want.

Geographers engage in a self-fulfilling prophecy every time they invoke an *ad hoc* hypothesis to protect one of their "theories" from refutation. *Ad hoc* hypotheses have been used innumerable times in modern locational analysis, just as they have been used more generally in the past. For example,

> *The normal cycle of erosion model of W.M. Davis, which was based on the concepts of 'the cycle' and 'evolution' was intended to be a true description of the developmental sequence that was thought to apply to landforms under certain specified conditions. A decisive set-back in empirical tests for the normal cycle of erosion model would . . . have placed the whole cyclical approach to the analysis of landforms in jeopardy. But with the insertion of the hypothesis that, at any point, the cycle might be interrupted the possibility that the Davisian model could fail an empirical test became almost non-existent (Guelke 1971:48).*

It was not that the Davisian model was so completely compelling. Rather, Davis was a very forceful advocate and held considerable sway within the discipline. Early criticisms of the Davisian model were made by Penck, Passarge, and Hettner; while some of these were not decisive in any clear-cut way, the criticisms were promptly attacked by Davis and his disciples (Davis 1919; Bowman 1926; James 1967:14; Tilley 1968). Moreover, critics of critics may simply have been invoking a common rule: "one counts one's hits but not one's misses" (Guelke 1971:48). At any rate, as James also notes, another reason the model may have persisted for so long is that criticism of sufficient volume to establish a dialogue was lacking (James 1967:14).

There are other examples of self-fulfilling prophecies that have involved geographic concepts and environmental realities. One has been discussed, though not labeled as such, by M.J. Bowden (1976). He notes that nineteenth-century historians accepted the idea of the existence of an authentic desert east of the Rockies. They were romantics, and quickly inclined to place "exclusive reliance on secondary and promotional sources, and an unquestioned acceptance of myths disseminated by the national gazeteers (M.J. Bowden 1976:125). More interesting and revealing than these isolated facts, however, was how historians and others in the early twentieth century turned previous conceptions on their heads. They minimized the role of the physical environment in their conceptual kitbags and thus had little need to claim that the public had a desert conception of the Plains between 1820 and 1870. Bowden argues that the reason the physical environmental variable was downplayed, thereby resulting in a "revision" of past perceptions, was the implicit view of the environment in Turner's frontier thesis, a thesis widely accepted by historians in the twenty or so years prior to the Great Depression (M.J. Bowden 1976:126). Turner's thesis was primarily a

locational theory emphasizing the remoteness and size of the Plains, its unique closed space on the American frontier, and the notion of westward overexpansion. Within this scheme, the physical environment could not be a major explanatory variable and therefore "if a distinctive Plains pioneer with unique problems emerged, this was largely a consequence either of his location in American space or of his inexperience on the American frontier" (M.J. Bowden 1976:127). In another context, then, geographers, in practice if not in name, saw what they could only have seen from the bench upon which they stood. The American desert in the mind of one American group was a reality; to a later group of observers that believed in Turner and his ideas it was a figment of everyone's imagination.

A final aspect of the self-fulfilling prophecy concerns pressures that influence the dissemination of results. People want to hear what pleases them. If those who are to receive the results are also funding the research or passing judgment on the researcher, then this increases the likelihood that the findings will be — at least in part — predetermined (Andreski 1976:39-40). One of the unspoken requirements of funded research (whether in physics or geography) is that there be positive results. The more striking or spectacular, or the more they are in line with the implied objectives, the more successful the researcher. And the more likely is funding in the future.

If self-fulfilling prophecies are a particularly worrisome occupational hazard of one who has a strong conception of the principle of order, it is not an exclusive worry. The cultural geographer/ecologist's adaptation bias is often as pernicious. Unless considerable care is exercised, in that controls are instituted to at least leave open the possibility that an adaptation hypothesis can be falsified, one is likely to be generating tautologies. The researcher searches for and finds traits that are adaptive, and does not look for and therefore cannot find, those that are maladaptive. The sin of finding what is presupposed beforehand in terms of a trait function also results in a perspective, indeed another assumption already mentioned, of stability or unchanging systems (Sopher 1972:322). A methodology can only accommodate that which is contained in its assumptions.

Strong preconceptions also have a way of making the purpose of methodological instruments ambiguous, or of giving them meaning which harmonizes with such preconceptions. In a geographic context, the use of models illustrates this point. There is plenty of evidence in both textbooks and research writings to the effect that geographers understand that there are various kinds of models and that they can serve various functions. What seems less clear is that model uses have been ordered in terms of how they ought to be used or appreciated; even less has been said about how models probably ought not to function. As an example, Mark and Peucker have recently noted that the least squares regression model should only be employed when the researcher is interested in prediction. But geographers have consistently used the model for explanatory purposes when they should have been making use of some form of structural or functional analysis. Of some forty papers examined by Mark and Peucker in social and economic geography, more than half misused one of the most common and simplest of quantitative techniques (Mark and Peucker 1978:51-64).

Models are used in two principal ways: as simplified representations of reality that purport to explain it — albeit selectively and incompletely — and as simplified representations of reality whose main function is heuristic. Because of the way geographers (like other social scientists) are forced to cope with a quite complex reality, initially by simplification and then gradual reduction of such simplicity, and because we have other

concepts whose main purpose is to explain rather than perform heuristically (specifically theories), it is not difficult to make a case that the primary function of models is, or ought to be, heuristic. One can argue that models relate to themselves, theories to the external world: "A model must satisfy only internal criteria; a theory must satisfy external criteria as well" (Coombs *et al.* 1954:25-26). As Chorley (1972:188) has noted:

> *. . . it is important to differentiate between models and theories. Analogy is often a fruitful source of suggestion for hypotheses for further inductive investigation, but alone it cannot 'prove' anything. A model becomes a theory about the real world only when a segment of the real world has been successfully mapped into it, both by avoiding the disregarding of too much information in the stage of abstraction and by carrying out a rigorous* interpretation *of the model results into real world terms.*

However models are seen or used, they cannot be all things to all people. Guelke's point is well-taken:

> *The new geographers cannot have their cake and eat it. Either models are regarded as a special type of theory with explanatory power (the view taken by Chorley and Haggett), in which case they must meet the minimum requirement of all scientific theories, or else they are considered to be strictly heuristic devices with no role in the explanation of real world situations (Guelke 1971: 52).*

That models in geographic context are frequently interpreted as explanations for phenomena as well as heuristic instruments may derive from a confusion about what models are, a confusion pervasive in the social sciences. However, more may be involved. The problem may also be tied to the strength of the ordering principle in the minds of too many geographers. Indeed, one would expect as much: heuristics is a means, an intermediate process; order is an end, and ordering as process is an imperative to hurry toward a final objective. At some stage one knows that models are heuristic; at a later date, one forgets. All but a few quickly become impatient with heuristics. Scientists are rewarded for answers, seldom — at least directly — for questions

3

Skepticism and Criticism in Geography

The reasons many scientists give little attention to skepticism and criticism run the gamut from the intuitively obvious to the unexpected. In the first instance, it is clear that a discipline will be no more critical of itself than it has learned to be historically. If scientists have primarily focused on getting on with their substantive research, and if these efforts have been isolated ventures that largely preclude charges of inadequacy — as has been characteristic of geography through most of its history — then there is reason to expect a poorly developed sense of skepticism. If, too, as a recent president of the Association of American Geographers has pointed out, there is a shortage of outlets in the discipline for the expression of opinions on intellectual matters (Mikesell 1978:16), then, quite apart from the fact that scientists may all be working on isolated farmsteads, skepticism as principle will not develop.

Intellectual Tribalism

If skepticism and criticism are related to a history of being criticized, they are also related to social and psychological circumstances. Very little has been written about geography's social structure and social linkages (W. Bunge 1961; Rushton *et al.* 1971:107; Hurst 1973; Whitehand 1970), but there can be little doubt that criticism and attitudes toward it are socially dependent (Zelinsky 1975:133). Being part of a social group means, among other things, accepting those who define the group, their strengths, their shortcomings, their idiosyncracies. Having group membership also means having a sense of individual and perhaps group identity, now and in the future. Some principles more than others enhance identities and advantages.

> *Every craft, every occupation — no matter whether shady or even downright criminal — gravitates towards the principle that "dog does not eat dog." There can be little doubt that the appeal of the "dog does not eat dog" principle derives its strength less from altruistic concern for fruitfulness of the work — except in so far as this makes life easier — than from the quest for collective advantage, be it pecuniary or honorific (Andreski 1976:13-14).*

In recent years philosophers of science have increasingly argued that science is a social process, that its social nature is relevant to both the construction and validity of

scientific theories (Suppe 1974). An extreme position, such as that of Kuhn (1962), holds that science as a process of skeptical reasoning is essentially a group illusion. Scientific communities are seen as self-perpetuating mutual admiration societies whose social systems stifle innovation and suppress disconfirming evidence. One need not accept this position, however, to note the biases produced by the social process. A belief in the growth of scientific knowledge and acceptance of the idea of a reality independent of scientists' conceptions of it do not require a denial of the social influences on how these conceptions are derived (Mitroff 1976). Indeed, one can readily distinguish that although two scientists may *interpret* reality differently, they nevertheless confront the *same* reality (Pratt 1978:63-68).

In geography it is obvious that there are distinct groups who interpret reality quite differently. The papers in the March, 1979 special issue of the *Annals* of the Association of American Geographers commemorating "Seventy-Five Years of American Geography" make this clear. The "Berkeley School" associated with Carl Sauer and his acolytes, the Midwestern regional geographers and their hero Hartshorne, the spatial analysts and the "masterful" Schaefer, and the "radical" geographers, among others, emerge as examples of what Campbell (1979) has termed "tribal" scientific communities. Each of these groups has provided and provides a conceptual framework, although differing in their clarity, to each of its members. Moreover, each of these groups is a self-perpetuating social system to the extent that maintaining the continuity of the group often transcends the task of refining, let alone questioning, the conceptual framework around which the group clusters. Campbell's characterization of "tribal" continuity is immediately appealing as an exposition of how groups maintain themselves and isolate their conceptual frameworks from skeptical inquiry:

> *A scientific community must recruit new members and reward old members well enough so that young recruits will be attracted to a lifelong commitment to the field and will justify the drudgery and painful initiation rites. Journals must be published, purchased, and read. Members must remain loyal to the group and not "defect" to other tribes. Jobs must be found for loyal followers. Social facilitators are needed to keep the group together and must be rewarded for this role, even if this means giving them scientific honors not earned by their cognitive contributions. The requirements of leadership for coordination and continuity may produce leaders whose decision-making power is used to protect their own social positions and their own scientific beliefs against internal challenge from young rivals. The deeply ingrained social custom of building ingroup loyalty by mobilizing hostility and disgust toward outgroups may be employed as a convenience (and perhaps even occasionally as a necessity) in maintaining group cohesion and continuity (Campbell 1979:185).*

The *Annals* issue referred to earlier contains many examples of the group maintenance and outlook-shielding mechanisms to which Campbell alludes (*e.g.*, Leighly 1979; Spencer 1979; P.R. Gould 1979; Hart 1979; Peet 1979; W. Bunge 1979). For example, Leighly writing of Carl Sauer and geography at Berkeley, states:

> *Sauer always disclaimed any intention of founding a "school" or of shaping students over a common last. He expected the individual to find his own objects of interest and his own ways of satisfying his curiosity.*

> *What we had in common we gained mostly from Sauer's attention to the past of geography. Of his seminars I remember only those on the history of geography. In these we learned to appreciate the antiquity, the continuity, and the dignity of an intellectual concern with the earth. We were thus immunized against infection by the numerous "new geographies" whose birth has been proclaimed repeatedly throughout the history of academic geography in the United States (Leighly 1979:9).*

The more youthful Peter Gould recalls, and also represents, the technique of mobilizing hostility toward outgroups which Campbell sees as important for creating and maintaining group cohesion. He writes of geography in the late 1950s:

> *. . . they were sometimes ugly and vicious times, characterized by a meanness of the human spirit and the threatened pastures of the traditionalist lashing out in defense of his comfortable establishment world (P.R. Gould 1979:140).*

And, though "acutely aware of the way a particular viewpoint can shape historical material," Gould continues later to castigate Sauer and other "traditionalists" for a "bumbling amateurism and antiquarianism that had spent nearly half a century of opportunity in the university piling up a tipheap of unstructured factual accounts" (P.R. Gould 1979:140). As a representative of the insurgent 1950's spatial analytic school, Gould caustically notes:

> *With the exception of one or two works of scholarship in historical geography, it was practically impossible to find a book in the field that one could put in the hands of a scholar in another discipline without feeling ashamed. Only when Bill Bunge's doctoral dissertation and Peter Haggett's synthesis [of spatial analysis] were published was there something that one could offer without apologies (P.R. Gould 1979:140-141).*

Documenting the existence of specific groups or "tribes," however, should not be seen as the culmination of a sociological analysis. No scientific community is isolated from the larger society. Science is influenced by the general social system in ways that undermine skepticism. For example, status considerations based upon the structure of the large society affect the internal status system of science. Campbell argues:

> *Given equal ability, it helps a young scientist's appointment, promotion, grant-getting, and publication to be well connected in the extrascientific real world. It helps if one has good manners and is cultured. It helps, too, if one's ideas support rather than oppose the dominant interest groups of the larger society. Likewise, it helps if one comes from a high-prestige university. All such contamination violates important norms of science which hold that the contribution to scientific truth should be the only determinant of status within science (Campbell 1979:194).*

A president of the AAG addressing his colleagues may feel reluctant to put the matter so straightforwardly, but leaves essentially the same, easily-translated message when he notes that the social behavior of scientists is not much different from that of other groups, and that science "is infected with much the same social stratification,

class, racial, ethnic, and sex biases, lusting after power, and interpersonal frictions found elsewhere in society" (Zelinsky 1975:133). Only a few of these biases and their effects have been documented (Horvath *et al.* 1969; Mitchell 1969).

Individual psychology may also be of some importance in discouraging a skeptical outlook and critical thinking. This is largely a *social* psychological issue. In all groups competitive people are a problem. Campbell again captures the essential point:

> *Cooperative people who defer to the majority, who get along and go along with others, and who hold the team together, get preferential treatment even if they are less competent. This is true of scientific communities too, contrary to scientific norms that encourage vigorous internal criticism even if feelings are hurt and norms demanding that competence rather than likeableness is what should count (Campbell 1979:194).*

The problem is compounded, however, by more than the psychological makeup of scientific communities, and by more than the fact that once a scientist begins to make use of a lens which shows him how to look for order, he cannot see any other kind. There is nothing inherent in the concept of skepticism that brings forth the novel or the new, or simply a replacement. Geography's quantitative revolution may not have ended as early as Burton claims, and a few geographers are presently satisfied with its solutions to a variety of problems. Yet, if there is any broader use to which Kuhn's concept of "normal science" can be put, then geography is presently practicing normal science. Kuhn's historically documented insight provides a window on the weak role of skepticism. That is, if one accepts the widely held, if erroneous, view that science has a built-in self-correcting mechanism, Kuhn reminds us that "no part of the aim of normal science is to call forth new sorts of phenomena; indeed those that will not fit the box are often not seen at all" (Kuhn 1962:24).

The Myth of the Self-Correcting Mechanism

A major reason skepticism is given relatively little attention by many is because of the rather wide-spread myth that science has a built-in self-correcting mechanism (Chorley 1972; Moss 1970). The common version of the myth is somewhat as follows. In science, methods as well as results are replicable. Therefore if a particular set of facts are not as claimed, this will quickly be brought to light as other scientists use a similar methodology and attempt to reproduce the alleged results. A conservative variant of the myth found among geographers and other social scientists is that while results are not or may not be replicable, methods are, and therefore this will lead to corrections within the corpus of knowledge. Toulmin, speaking for the physical sciences, voices a similar point of view: "So it is not Nature that is Uniform, but scientific procedure; and it is uniform only in this, that it is methodological and self-correcting" (Toulmin 1960:148).

Science as a self-correcting mechanism is sometimes depicted as a flow diagram in which images of a real world structure are modeled, translated into hypotheses that are fitted to an experimental design, followed by the collection of data and subsequent verification procedures, and then fed back to real world images or into law and theory construction (Harvey 1969:34). According to the model, either acceptance or rejection of the hypothesis results in a recycling of image, model, and hypothesis. Accepted

hypotheses are first integrated into theories before they are recycled, whereas rejected hypotheses are recycled — in theory — directly. Variants on this depiction of the scientific model are many, but its circular self-correcting character is a common theme (James 1972:472).

Somewhat like the principle of order, the self-correcting aspect of science is often only weakly tagged. The claim may be simply that science does a better job of weeding out its mistakes than do other ordering systems. A similiar claim is implied in the assertion that the answers science produces are replicable (Abler *et al.* 1971:19), which, since little is said about the nature or difficulties of actual replication, leads one to conclude that replication is, in fact, common.

A self-correcting mechanism is something of a reality in certain areas of the hard sciences. Everyone is aware of the shame showered on the cancer researcher who claims new and significant findings and whose results cannot be replicated. Falsified or fudged findings are thrown out, and the mechanism seems to have worked as claimed (Wade 1976; S.J. Gould 1978). But the glaring example does not prove the case, and even in the hard sciences where theories are reasonably well-developed and interconnected, and the logic of mathematics assists the corrective process, the picture is murky at best.

Once one enters the social sciences, and also many areas of the biological arena, the problem of preconceived mind-set is compounded by several other issues which, taken together, make the concept of a self-correcting mechanism appear little short of empty. One problem in the social sciences and the softer sides of the biological sciences is that methods are, in principle, replicable, but results often are not, except in the most general and trivial sense. This is so in the study of social change in the same system, to say nothing of notable cultural variations over time and space. Another problem is that methodologies are usually insufficiently specified in published papers. Thus, in many instances even if the results were replicable in principle one would not expect them to be identical for two different researchers looking at the same system at the same time. Moreover, because so few social science results are backed by careful documentation and statistical enumeration, and because so many models and "theories" possess an indeterminate logic (in that ordinary language rather than mathematics is the medium for expressing theory and results), what would constitute a contradiction of a given result is equivocal. Fourth, surprisingly few attempts are ever made to replicate a given data set using the same methods, or a set of results using any method whatsoever. Even a widely discussed and — for many — widely accepted relationship such as that alleged to hold between complexity and stability in ecosystems has been subjected to surprisingly few empirical tests. Most social scientists, and many biologists, prefer not to re-examine what others have done. Rather, they insist on cutting a new swath through what they believe to be virgin territory, no matter that most of the original growth was cut away long ago. The reason is simple: scientists give few kudos to colleagues who march over once-trodden pathways (Merton 1973). One must at least give the illusion of doing something new.

Fragmentation and lack of replication is particularly severe in geography which, by definition, is so sprawling an effort. Its small membership and limited research output of most of its practitioners compound the unlikelihood of replications. If one is hard put to see the self-correcting mechanism at work in various subareas of biology, it is difficult to argue within geography that its existence is other than an illusion. Could it be that there is less difference between the authoritarianism of religion and that of science as practiced in some disciplines than we have been led to suppose? It is an au-

thoritarianism that comes not from official decree, but rather from the reification of once-tested theories and hypotheses; with time, they take on the appearance of, and are presented as, well-confirmed generalities.

Other Correlates of Skepticism

Notwithstanding Kuhn's observation that rejection and acceptance of an alternative paradigm are simultaneous processes, one might expect some correlation between the degree of theoretical and conceptual development and the prevailing attitude within a discipline toward skepticism and open criticism. A discipline with well-developed and broadly based theories with numerous empirical referents, or with substantive studies widely recognized for their insights, even if not particularly theoretical, has a much larger and deeper cushion with which to absorb internal and external criticisms than is true of a discipline with opposite characteristics. In the latter case it would be somewhat surprising if practitioners did not rail against efforts to question central theories and findings and, equally surprising, if the discipline encouraged open criticism. To say this is to say little more than that scientists are human. To quote John K. Wright (1966:54), ". . . among the most valuable of the lessons to be learned for the history of science are those concerning the ways in which science had hitherto reflected human nature and will doubtless always continue to reflect it." Scientists want to protect what they have, particularly if they do not have very much.

Theoretical Weakness, Disciplinary Self-Deprecation, and Status

Geography has a poorly developed posture toward skepticism in part because of the relative weakness of its theoretical and substantive base. It is a commonplace observation within and outside the discipline that geography is relatively impoverished theoretically and substantively as compared to, say, economics. One widespread measure is the extent to which geography is often excluded or ignored by other social scientists when designating the panoply of what constitutes the social sciences or, more specifically, its accomplishments (but see Berelson and Steiner 1964).

Geography's perceived secondary status can be noted in public image and in the rather widespread attitude of apology toward disciplinary findings, learned early in one's graduate education and a continuing theme as one ages (Dury 1970; Blackbourn 1966; Alexander 1959; Borchert 1960; Cohen 1962). Reminders run the gamut from the overt to the subtle. More than twenty years ago, Van Cleef, in one of the discipline's major journals, summarized his feelings as follows:

> *The work of many generations of geographers is still necessary merely to establish a foundation upon which further construction may take place. Geographers must observe every square inch of the earth's landscape hoping to find enough repetitive cases to enable them to convert hypotheses into law. For this reason it would seem we need not apologize for our still primitive stage, if we will be frank to acknowledge our deficiencies. We need only confess that we are still evolving; that because an important part of our subject matter, the human, is highly variable, we have a much longer road ahead and a much more difficult one than that of our colleagues in the physical sciences. If we take this humble position, the world as a whole is more likely to view us and our work with greater charity and a higher regard (Van Cleef 1955:108).*

Twenty years later a president of the AAG, speaking before an assembly of his colleagues about "demigods" in science, practically ignored talking about his own discipline (Zelinsky 1975). At one point in his presentation, Zelinsky made a comparison between the finest minds in physics, chemistry, and medicine and how they might compare with those in the social sciences, mentioning that those in anthropology, economics, and psychology are on a par with 'hard' scientists. But geography and geographic minds, those presumably addressed, were ignored in the comparison (Zelinsky 1975:137).

By comparison with many anthropologists, most geographers are naive and reluctant participants in the arena of active intellectual criticism. Why, one might ask, has none of the major geographical journals instituted a format similar to that employed by *Current Anthropology* where a major article of apparent significance is submitted to a large number of scholars prior to publication, with the explicit intent of criticizing weak points and high-lighting those that deserve elaboration? Whether the answer proffered has to do with the quality of geographical writings, with an unwillingness to break with tradition, or with a negative attitude toward such open criticism matters less at this juncture than the point that the discipline, taken as a whole, does not place a high value on criticism.

Two special cases of the previous points require attention. One involves the strength of the taboo against criticism, the other the relationship between relative status of skeptic and the person whose work is criticized. On the first issue, given that we have a discipline weak theoretically and substantively, it would seem that the more central, fundamental, or widely accepted a particular theory or substantive finding, the stronger will be the taboo on criticism with regard to the theory or finding. To use a metaphor, it is as if the critic is trying to dislodge the bottom-most block of an inverted pyramid. It is not that the entire structure will, in fact, crumble; rather the strength of the taboo may simply be related to the fear that it may. This hypothesis might well be separated so as to make a distinction between a theory or substantive finding that is central or fundamental and one that is widely accepted. A variety of examples in geography show that what is fundamental — by a criterion of theory construction — may not be widely accepted, and vice versa. From one domain of the discipline one can cite the fundamental, if not yet widely accepted, entropy maximizing principle (P.R. Gould 1972; Wilson 1970). From another area, one can note the purely classificatory regionalization scheme of core, sphere, and domain, widely accepted and used by cultural geographers but hardly now fundamental or ever likely to be (Haggett 1975:244; Zelinsky 1973: 114-16; Meinig 1965; 1969:110-24).

The relationship between the relative status of critic and the person being criticized may be confounded by several variables, but not enough to vitiate the hypothesis that the greater the difference in actual or perceived status between two scientists, where the critic is of lower status than the person criticized, the more likely it is that the reaction to criticism will be negative. Once again we are dealing with a widespread human tendency, that of linking both authority and closure on an issue to status. One recorded case which supports this hypothesis occurred in the IQ arena. Kamin, a psychologist who was an outsider on the IQ issue until 1972, criticized Sir Cyril Burt's data on inherited differences in intelligence, describing them as "simply not worthy of serious scientific attention." Despite the fact that his charge was grounded in some straightforward, common sense observations regarding data inconsistencies and some highly implausible correlations, Kamin was roundly criticized by another student of the heritability question because "Burt was a towering figure of 20th century psychology . . ."

and ". . . it is a crime to cast such doubt over a man's career" (Wade 1976:916-17). In the context of human geography, most of the criticism Szymanski personally received for his critique of Meinig's use of language in historical geography was of precisely this sort (Symanski 1976); it was not substantive. In a more general vein, Merton had few doubts about the role of status in the context of criticism. He stated that he who assembles the facts against the work of great men is considered a raker of muck, while "to investigate the subject systematically is to be regarded not merely as a muck-raker, but as a muck-maker" (Merton 1976:39).

A corollary of the hypothesis pertaining to status is that while the reaction to criticism may not be particularly negative, emotive reactions may, nevertheless, be more in evidence when there is a notable status difference. One difficulty in testing these hypotheses, apart from the number of occurrences that arise and are recorded, is that they may be inter-correlated with the hypothesis concerning the centrality or widespread acceptance of a theory or finding. And there are other possible intervening variables, which include the nature of personal ties among those directly involved in a critique and among these individuals and others in the discipline not party to the disagreement but who nevertheless feel obliged to offer arguments — substantive and *ad hominem* — either about the form of the argument or its propriety.

Skepticism and Originality

At least one other correlate of skepticism needs to be considered, namely the connection with originality. Polanyi claims that "the value attached to originality encourages dissent . . ." and ". . . that this internal tension is essential in guiding and motivating scientific work" (Polanyi 1962:58). If Polanyi's observation has generality, then, based at least in part on preceding arguments, the expectation is that geography does not place much emphasis on originality. Is this so? The question is, frankly, very difficult to answer, primarily, it would seem, because the discipline does not possess well-defined criteria as to what constitutes originality.

An argument to the effect that criteria in fact exist and are employed all the time by journal editors and referees is not convincing. What gets accepted for publication is a complex function of who wrote the paper, the attitude of the editor toward the topic; and his weighing what referees say; the extent to which the paper is "too hot," "too controversial," or "not geographic enough"; and, of course, the quality of the message or finding. Even if it were argued that the latter is the most important criterion — which in many cases seems doubtful — the question of originality still remains. To put it simply: what is originality? It is certainly not the same as any model modification, another case study, or any synthesis. And it is not conferred simply because of publication, number of citations, or controversy stirred. A nod for originality surely goes to Christaller, but then not unqualifiedly as recent textbook authors point out (Berry *et al.* 1976:228). Vance's "mercantile model" appears original (Vance 1970), but can his originality be compared with that of Christaller? And what of Skinner's widely cited application of central place theory to market-places in rural China (Skinner 1964-1965)? Was he original at all, despite the alleged importance of his work (Murphey 1973; Soja 1973)?

One way of addressing the question of originality is to pose it in terms of the emphasis that scientists give to running ahead of the pack, to being first in making some particular discovery, and to guarding jealously their claims to priority (Medawar 1979). Physical scientists have traditionally paid tremendous attention to priority claims. Medawar writes that "disputes over priority, sometimes of an especially venomous and

unforgiving kind, are as old as science itself" (Medawar 1979:41). Artists, however, "are a little contemptuous of a scientist's anxiety for credit. . . . The 20 years Wagner spent on composing the first three operas of *The Ring* were not clouded by the fear that someone else might nip in ahead of him with *Götterdämmerung*" (Medawar 1979:41-42). Geographers' attitudes to priority are more like those of Medawar's "artists" than his physical scientists. But they are not novelists or composers. In geography there is none of the constant urge to pursue and substantively defend one's originality (priority) that one finds in the physical sciences. This may discourage criticism and skepticism (Merton 1973; Gaston 1973).

The question of originality obviously has no simple answer and any attempt to discuss it adequately goes well beyond the issue raised here. What is germane, perhaps, is that the very difficulty any of us have in talking about originality may be enough to suggest that its relationship to skepticism and dissent is unclear. That work *defined* as "original" tends to become institutionalized and beyond reproach is clear.

The Exception to the Generalization

The major exception to the generalization that geographers do not have a strong sense of criticism arises in an ideological context, generally on the question of values (Buttimer 1974; Entrikin 1976; Tuan 1976), and specifically on issues that relate to exploitation within the existing capitalist social order of the western world (Harvey 1972a, 1972b). The central case within geography involves radicals, primarily those who fashion themselves as Marxists and, more generally, those who identify with the variety of left-radical positions found in the journal *Antipode*. In a sense, one would expect Marxists to be the most critical group within a discipline. One reason is a basic philosophical belief that the very methods espoused involve continual criticism, not only of the existing social order but also of themselves (Harvey 1973:88). Their emphasis on exploitation, the class struggle, and contradictions within capitalism is no mere theoretical matter; they feel compelled to combine theory and practice — ideally, at least — and if this does not translate to community work, it does find expression in their attacks on the discipline. A third reason for criticism relates to their minority status within the discipline. Many of the Marxists feel oppressed by the "establishment"; many have been; and their understandable, logical reaction is to lash out.

Marxist criticism has at least two major dimensions. One is an attack on the capitalist social order; the fundamental interest is not how the world is, but rather how it ought to be. This often leads them to commit the normative fallacy, the confusion of "is" and "ought" statements. It cannot matter because, to them, it is more important to change the world than to know it.

A second sense in which Marxists practice criticism is toward the discipline. They see it as reflecting the class divisions within society, and, with its emphasis on positive science, fundamentally irrelevant (Hurst 1973). Geography is seen to perpetuate the oppressive, exploitative status quo; its models and methods are counter-revolutionary (Folke 1972; Harvey 1972b), not inherently so, but "they become so in the course of their usage" (Harvey 1973:86). Thus, as self-perceived revolutionaries, it is not surprising that geography's Marxists should speak of the need as:

> *. . . teachers-workers to struggle for control of the means of producing and distributing ideas. We must build . . . means of production based on socialist values, radical analyses and the lessons of revolutionary*

> *action. More concretely, we must criticize conventional ideas, expose their weaknesses, biases and utility to the capitalist order, and must produce alternative and more persuasive theories based on the radical paradigm (Peet 1974:iv).*

Their criticisms have not gone unheeded, and they have affected some of those most strongly identified with positive science within the discipline (King 1976).

To be uncritical is to be non-Marxist, Marxist of the wrong orthodoxy, non-radical, or, worst of all, "establishment" (Golledge 1973; Slater 1977a). Sometimes the meaning attached to "uncritical" by Marxists is followed by convincing critiques (*e.g.*, Gregory 1978); sometimes criticism is little more than a superficial form of religious cant, exemplified in the use of such words as "mystification," "contradiction," and "naive empiricism."

The posture, in principle, is healthy, but the results often are not. When a critical exchange occurs between an avowed Marxist, such as Harvey, and a member of the establishment, such as Berry, the result may be more a parody of intellectual exchange than anything else. Berry's review of Harvey's important book, *Social Justice and the City*, is described by Harvey as a "formula review," so deplorable that he knows "several editors who swear they will never send another book to Berry again because of it" (Harvey 1974:145). He finds Berry's review "bewildering," "somewhat obscure," containing innuendo, and ultimately, a piece of "political rhetoric," which he recognizes is also true of his own criticism of Berry (Harvey 1974:145, 147). Finally Berry is accused by Harvey of defending the "ruling ideas of the ruling class" (Harvey 1974:147). Berry's reply to Harvey's criticism and accusations degenerates into cliches about "flower-power" and what everybody knows to be the parallel between Harvey's reply and English schoolboy tricks about winning debates (Berry 1974:148-149). In the midst of this "critical exchange," Harvey quotes at length from an anonymous review of his book in the *Times Literary Supplement* (Harvey 1974:148)!

Enough evidence is not yet in to know whether exchanges of the Berry-Harvey sort will be typical of those representing radically different ideological positions. What the exchange may suggest primarily is that when the ideological lines are so sharply drawn, criticism may, in fact, degenerate into pseudo-criticism. "Each radical geographer may agree on the necessity of a socialist transformation of society, but true to the internecine history of the Left, we read each other out of the fold, rather than work to define a feasible future and how to get there" (Morrill 1973:84). Both parties have decided *a priori* that their positions are correct and thereby there is nothing to learn from a critical exchange. The object of the exercise for both participants is to score an advantage for one's political position. That the same occurs with regard to one's vested intellectual interest within positive science is not to be denied; whether or not there are significant degrees of difference between political and intellectual ideology is a moot point. Indeed, one so imperceptibly merges into the other that they are as hard to separate as are the personal backgrounds and biases of those engaging in supposedly intellectual exchanges.

There is no question that open criticism of a radical analysis is a more accepted part of the radical geographer's way of doing geography. This is reflected in the consistently large number of articles in *Antipode* that are directed to previously published pieces, in stark contrast to any of the discipline's other major journals (Kongstad 1974; Friedman 1974; Ive 1975; Wolf 1977). Some are directed to clear-cut extensions of Marxian analysis, some to whether or not the interpretation is unorthodox or vulgar, a

common type of criticism in the Marxist literature (Blaut 1978:96). But some criticism concerns the question of whether or not it is a good idea to break within the ranks by even criticizing fellow revolutionaries. For example, in a critique of numerous geographers by Slater (1977b), one of whom is Blaut, the latter is upset because a presumed comrade has ignored the "canons of scholarly and comradely discourse." More generally, for Blaut, "it would seem reasonable to suggest that debates among Marxist geographers present in the pages of *Antipode* should be conducted on a more serious and comradely plane — than the one chosen by Slater" (Blaut 1978:96). If Blaut is seemingly more upset than the average scientist that, as Andreski puts it, "dog does not eat dog," it is probably more a function of wanting to present a united front from the Marxist ghetto within geography than anything else. Even in an intellectually promising context, therefore, tribalism often wins out over skepticism.

Skepticism and Criticism

We have argued that geography does not possess a healthy attitude toward skepticism and criticism. Although there are a variety of reasons for this predicament, fragmentary evidence suggests that the nature of social relations plays no small role. Within the left and among a relatively new group of radicals within geography, the attitude toward criticism seems more positive. However, even here there is a suggestion that criticism will be tolerated only in so far as it is leveled at the "establishment" and the existing social order. Prominent Marxists within the discipline would clearly prefer that criticism within their paradigm be kept at a minimum.

Were science in fact a self-correcting enterprise, as many claim and many more believe, the impact of social relations or ideology on critical posturing might be less. We have argued that in a discipline with weak theory and small output, and where a premium is not placed on "originality" — as is true not only in geography but elsewhere in social science — the notion of a self-correcting mechanism is a myth. Subsequent chapters provide evidence for this allegation, as well as for the more general charge that most geographers do not hold to the idea that order and skepticism ought to be dialectical.

4

The Case of The Universal Hexagons

Most social science results have their greatest impact in the classroom. Students are the largest and perhaps most important captive audience to which social scientists have access; it is students, as individuals, who will not only communicate to others outside the university what they have learned, but in a number of cases, they will attempt to put their blackboard and textbook lessons into practice. For these reasons, the numbers involved or affected indirectly and the potential decision making capabilities of students, it is the duty of the social scientist as teacher to insure that his words and examples reflect a balanced menu between the order he believes to be representative of some domain of knowledge and a skeptical approach toward such order. The latter extends not just to the issue of relating how and why such order is imposed, but also to the question of how representative such order is for the domain in question. In short, scientists have as much an obligation to be careful in the classroom as they do in their research efforts (Merrens 1966).

There is no simple way of knowing how fair scientists are to their students, or what impact they ultimately have. Yet, there are means of measuring, if ever so crudely, what in fact is given to students. This can be accomplished by examining the contents of textbooks, which purportedly represent the accumulated knowledge of a discipline at a point in time.

If one accepts the proposition that equal weight ought to be given to classroom and research obligations, and the further assumption that textbooks do have an impact on students' views of the world, then it seems logical to conclude that textbooks should be as carefully written as are research papers. Yet, there is considerable evidence — albeit more talked about informally than written — that most textbooks do not meet standards imposed for research papers, not even in the simple sense of accurately representing published results or giving a representative picture for a domain of inquiry. It is easy to blame textbook authors for this state of affairs, but they are not solely at fault.

It is widely professed that one's peers give few rewards for writing textbooks. Indeed, there is a feeling among some that one's reputation can actually suffer for having written a book for classroom use (Merrens 1966; Thrower 1970). Although few authors will record the aspersions likely to be cast, a few do. Boyce noted an illustrious colleague's reactions to the idea of writing a book on economic geography: "Anyone

would be a fool to undertake such a work," among other reasons, because "it would be particularly wasteful for a scholar to write a textbook since it would take valuable time and energy from 'research' " (Boyce 1974:vii).

However one might wish to place the blame for the sorry state of textbook writing, it cannot be excused. Nor can one easily argue that textbook writing does not matter, and that, therefore, the dialectic between the principles of order and skepticism ought to be less operative. Indeed, another major reason that particular care is important is that "no scientific falsehood is more difficult to expunge than textbook dogma endlessly repeated . . ." (S.J. Gould 1978:504). Textbooks, if not wholly at least in part, are written from other textbooks and from notes whose primary function was other than careful and accurate representation of the original source. And the very nature of a textbook is such that it suggests widespread acceptance of the material covered.

The first of our case studies is set within the classroom-textbook context. We examine the use of one particular set of widely used findings on periodic and daily markets in the Third World. Our specific focus is on how such results are portrayed, and the sense in which they are misleading. Our conclusion is that textbook writers in geography overemphasize the principle of order, underemphasize criticism, and, therefore, improperly transmit information.

Periodic Markets

Most writing about periodic and daily markets in the last fifteen years falls under the broad rubric of what Kuhn would describe as normal science. Hypotheses have been tested and related to existing theory; missing or incomplete theoretical niches have been sketched and, in some cases, elaborated in detail; and operational and non-operational models have been borrowed from elsewhere in science and examined for their applicability (Bromley 1971, 1974; Bromley and Symanski 1974; R. Smith 1978). Objectivity in approach and universalization of results have become accepted norms. Progress is evident in the sense that today more is known about the range of certain empirical generalizations, and more is known about the particular kinds of data that need to be collected for theory verification. There are even signs that the hegemony of central place theory in the study of periodic and daily markets has eroded, and that a "paradigm crisis" is on the horizon. For the past two decades, the theoretical study of Third World internal marketing has been pervasively influenced by classical location theory and economic reasoning (Stine 1962; Good 1972; R.H.T. Smith 1971; G.W. Skinner 1964; Hay 1971; Webber and Symanski 1973; R.H.T. Smith 1978). There are signs that non-economic modes of analysis are becoming increasingly important (Bromley, Symanski, and Good 1975; R.H.T. Smith 1976; Cook and Diskin 1976). Whatever the real impact of these new horizons for the study of periodic and daily markets, the bulk of research has been exemplary of research clusters throughout the social sciences. Everything seems a bit boring to those who do not appreciate that major breakthroughs are few and far between; that additions to knowledge appear insignificant when examined at short time intervals; and that the normal "new" finding is really not that new or exciting. But such is the nature of normal science.

It is upon this stage that we draw attention to the now famous Skinnerian hexagonal diagrams for pre-Communist Szechwan (*Sichuan;* G.W. Skinner 1964). As judged by appearance in geographic textbooks, by attention of students of periodic and daily markets, and by publicity in pan-scientific journals, Skinner's findings in general,

and the pictured hexagons in particular, are easily the most publicized aspect of periodic market research (C.A. Smith 1974; R.H.T. Smith 1978; Soja 1973; Johnson 1970; Berry 1967; Murphey 1973; Plattner 1975).

Skinnerian Hexagons

The mention of periodic markets in textbooks, either as socioeconomic institutions common to Third World countries, or in their relation to a theoretical discussion of central place theory, almost invariably draws forth examples from G. WIlliam Skinner's study of markets in rural China (G.W. Skinner 1964:3-43; Lloyd and Dicken 1972; Kolars and Nystuen 1974; English 1977; Haggett 1975; Berry *et al.* 1976; Berry 1967). Skinner's study may be the only one illustrated, particularly if the discussion revolves around the shape of the market area — which it usually does.

Of three Skinner articles one alone is usually cited (G.W. Skinner 1964:3-43). Skinner (1964-1965) purports to demonstrate that there is a small, "transformable" discrepency between reality and central place theory predictions of market spacing. Skinner's transformations — reality to model pattern — are diagrammatic and without mathematical basis. This is not to say that the hexagonal networks of central place theory are unfounded, but, rather, that no set of equations we are aware of, and certainly none presented by Skinner, describes the change from Chinese reality to his transformed cartograms (Figure 1). Skinner's transformations, quite contentious from our point of view, are done twice, once for a set of nineteen market towns, and a second time for fifteen in a nearby area (Figure 2). In short, he attempts to demonstrate that he has found a real world example not too unlike that predicted by a significant social science theory, and that the gross residual between reality and model can be taken care of easily enough through a process of inserting unsubstantiated market area boundaries and a rearrangement of the relative location of market towns.

If a geographer feels pleasure at what Soja has claimed to be "one of the most insightful empirical applications of central place concepts in the literature" (Soja 1973:168), and if the university lecturer who teaches the material is rather easily convinced by the neat fit between theory and model, then it is easy enough to imagine what students conclude. They conclude that hexagonal market areas are a geographic reality that can be explained by an important geographic theory. And they quite likely conclude, since statements to the contrary are invariably absent, that hexagons are not merely found in a China of thirty or more years ago, but that they are also found there today, and elsewhere, now and in the past, around the world. A case study, students conclude, is representative of a world of hexagons.

Besides an occasional textbook reference to the widespread and equally important phenomenon of periodic market cycles, there is usually little or no mention of other studies that either support or contradict the patterns discussed by Skinner (Stine 1962). This lopsided attention to Skinnerian hexagons cannot be explained by the rarity of periodic market studies or their inaccessibility, for there is a huge, almost unmanageable literature on the subject (Bromley 1974). Why then is so much attention given to Skinner's study? Our answer is the heavy emphasis that geographers place on discovering or demonstrating order. Textbook writers do not ask themselves whether the Skinnerian hexagons are the general or average case, whether the transformations make sense, or whether they have provided enough information so that students will not draw unwarranted inferences about the model, depicted patterns, or the generality of the case. One of the principal arguments that some might raise in defense is that

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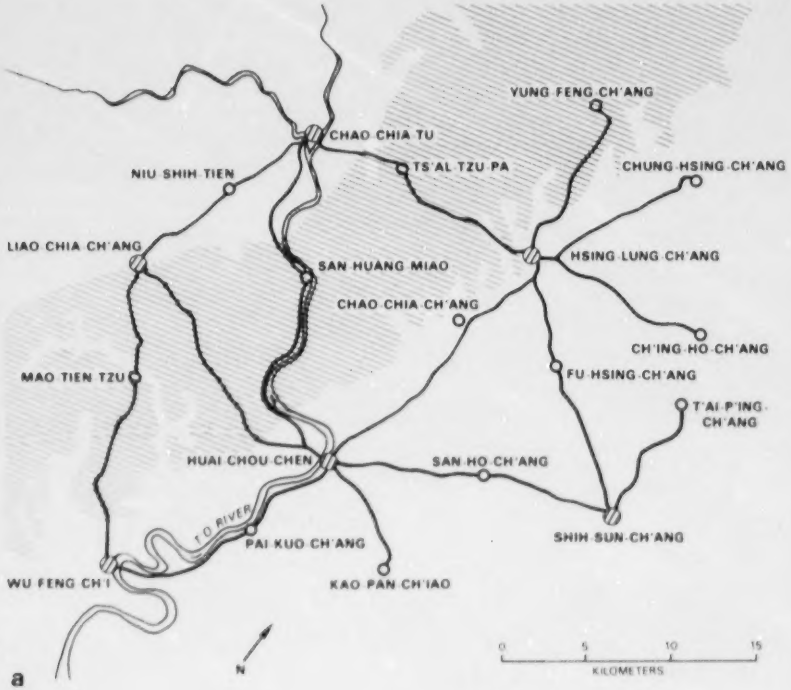

FIGURE 1 SKINNER'S MODEL A. Redrawn with permission from G. William Skinner, "Marketing and Social Structure in Rural China," *Journal of Asian Studies*, 24 (1964).

Skinner's material is used only to illustrate a theory or principle, and other examples would require more space than is available. But this kind of argument begs the question of proper and responsible transmission of knowledge.

The Problem of Unreported Data

The shape of consumer market areas can be delimited by finding out where consumers live and where they actually do their shopping. Without these or similar data, it is not possible to know with any precision the *shape* of the market area surrounding a market town. Skinner could, of course, examine the pattern of settlements, a quite useful exercise, based on the not unreasonable assumption that consumers go to the nearest market center. But Skinner's task, of particular interest to those who have found his work worth citing, was to show that "the service area of each market should approach a regular hexagon" (G.W. Skinner 1964:17). If he could not show this, then he could not establish a fit between Chinese reality and the hexagonal market areas predicted by theory.

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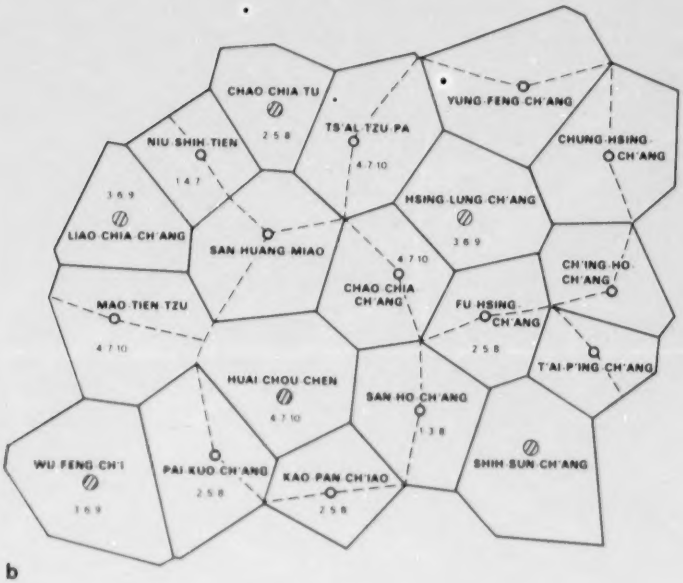

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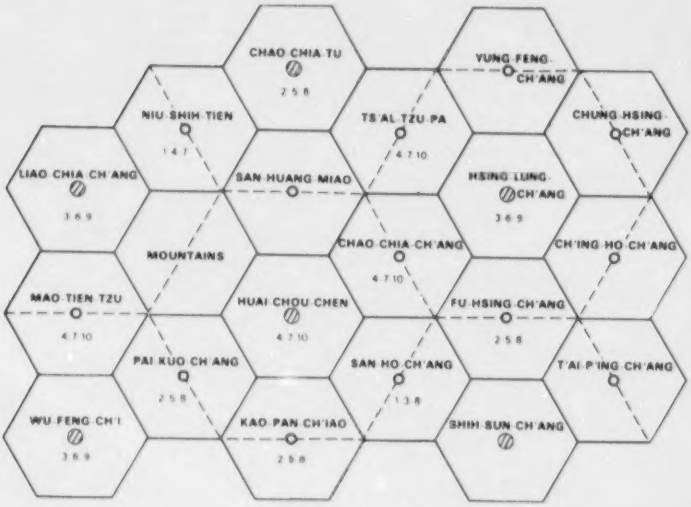

c

KEY:

- Limits of Standard Marketing Areas
- Limits of Intermediate Marketing Areas
- Roads Connecting Standard Markets to Intermediate Markets
- O Standard Market Towns
- Higher-Level Central Places
- Mountains Above 500 Meters

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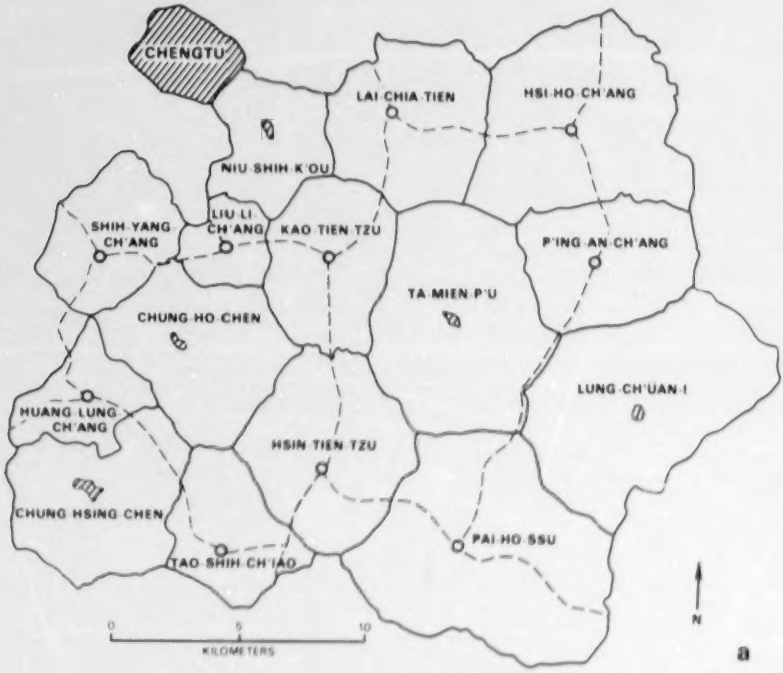

FIGURE 2 SKINNER'S MODEL B. Redrawn with permission from G. William Skinner, "Marketing and Social Structure in Rural China," *Journal of Asian Studies,* 24 (1964).

Skinner provided no numerical data, whatsoever, in regard to the location of consumers or the markets they frequent. The most he could say directly on this issue was that:

> *. . . in six areas of China where I have been able to test the proposition, a majority of market towns have precisely six immediately neighboring market towns and hence a marketing area of hexagonal shape, albeit distorted by topographical features (G.W. Skinner 1964:17).*

Of course, it does not necessarily follow that because a market town has six immediately neighboring markets that its marketing area is hexagonal. The latter is a separate determination. Also, Skinner did not state how he tested the proposition, which six areas he tested, how many markets were involved, or the nature of areas tested which did not have six neighboring markets. Concerning the last point, it is well to repeat Guelke's aphorism that "one counts one's hits, but not one's misses." A little later in the same section, Skinner, drawing on an example from a single marketing area

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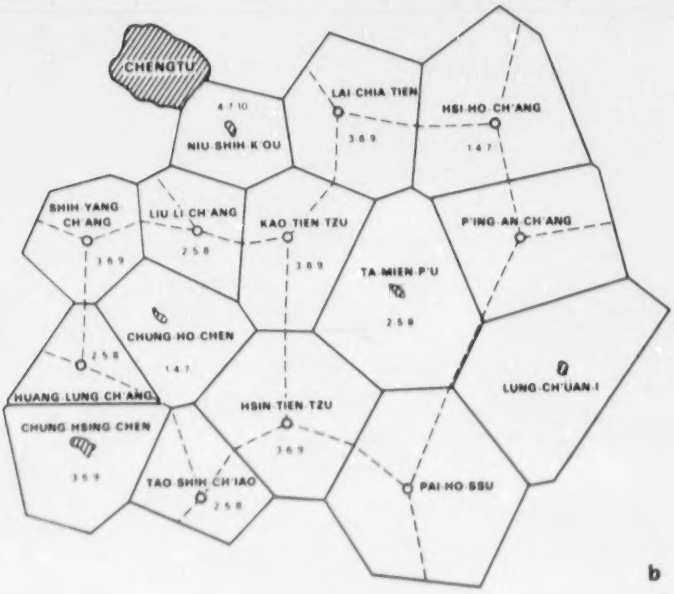

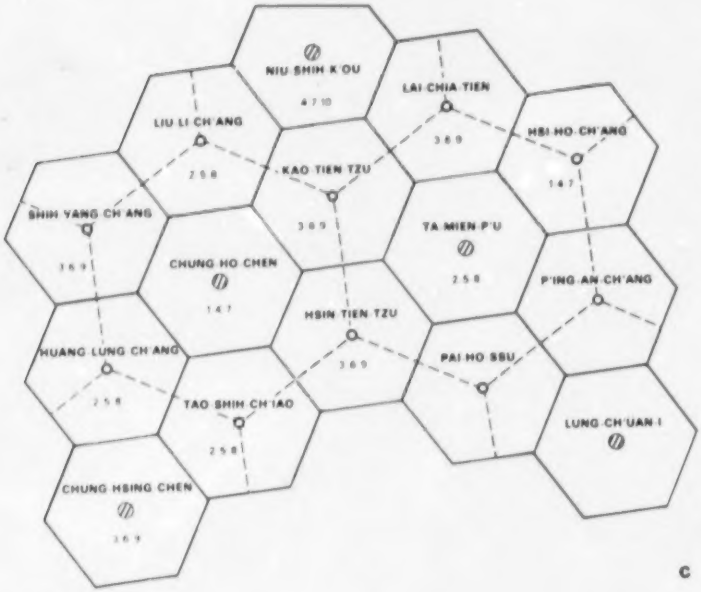

KEY

——— Limits of Standard Marketing Areas

– – – – Limits of Intermediate Marketing Areas

O Standard Market Towns

Higher-Level Central Places

he was studying, noted that he had little trouble in ascertaining the limits of the marketing area. But even with regard to this singular case, he neither presents data on where consumers lived and marketed, nor does he state that the market area approximated — taking into account distortions due to topographical features — a hexagonal shape. Yet, in a footnote accompanying this discussion, he recorded the fact that "the only study of rural marketing in China which refers to the shape of marketing areas insists that they '. . . approach circular or square form' " (G.W. Skinner 1964:17 citing Yang 1944).

In a lengthy footnote far into the body of his article, Skinner noted that "most of China's standard marketing areas are far smaller than any consideration of walking distance requires . . ." (G.W. Skinner 1964:33). This statement was a reaction to Yang's study, decades earlier, indicating that "walking distance" was a determinant of marking areas. Again Skinner presented no data, but if he was right, then there may be even less reason to believe that any surrogate measure of market area shape, such as the number and location of lower-order market towns, can be effectively used to compute the geometry of service areas. This obtains since peasants might well walk to more distant markets if the marketing areas are *not* constrained by walking distance.

In addition to these various problems, Skinner provided no surrogate statistical data which might suggest that the market areas approximate the hexagonal. He does not, for example, give figures to show his method for discriminating among standard, intermediate, and central market towns. Skinner does not inform us how his hierarchy levels were defined. There is no size ranking of market centers, there are no evident ambiguities and anomalies (where are they not present in the real world?) and there is no indication whether his maps include all central places. Indeed, Skinner does not show market centers on the periphery of his study areas, just blank spaces. Surely markets exist on the "periphery," and most probably they have some relationship to those depicted. Why have they been excluded? In a word, Skinner should have informed us on such matters, especially in view of the "spectacular" nature of his findings. These issues lead us to question how the assignment of market centers to hierarchical levels, and even the choice of which market centers to include in the study areas, was done. Because examination of possible hexagonal arrangements could have suggested which patterns of market towns would be most likely to give the desired results, we cannot help but ask if this "classic" study is another example of the self-fulfilling prophecy.

For all of the assertions, generalizations, and omissions, we have little insight into Skinner's methodology and data. We are forced to rely on his statement that:

> *. . . data for the present study are drawn from my own field work in Szechwan, 1949-50, intensive interviews with a handful of émigré informants in the United States, Hong Kong, and Singapore; a large number of* fang-chih; *and a variety of other published works (G.W. Skinner 1964:4-5).*

The *fang-chih* are gazeteers for local areas that "provide information in remarkable detail about local markets" (G.W. Skinner 1964:4), but just what is included in the way of market town sizes and consumer travel data is not mentioned. In passing, it should be noted that in Part II of his study Skinner leaves the impression that he has "hard data" for Szechwan, which he says he does not have for the rest of China (G.W. Skinner 1965:227). Whatever the actual shortcomings of his data base for Szechwan, Skinner's generalizations for the rest of China are therefore more suspect.

It is a presumably well-accepted canon that textbooks present the accumulated wisdom of a science, and that such acceptance derives in good part from the fact that the "wisdom" is well-founded in data, facts, or "good theory." Geographic textbooks accept Skinner's hexagonal market areas as fact. They do so by ignoring the problems just discussed. They say nothing about Skinner's data base, nor the fact that if the market areas are to have empirical meaning, then such meaning resides in the actual location and spatial behavior of consumers, not in the presumed and unsubstantiated. One pair of textbook authors includes Skinner's hexagonal transformation pictures with brief and misleading descriptions in a chapter entitled, "Empirical Evidence of Spatial Order" (Lloyd and Dicken 1972); other textbooks also use his depictions as evidence of universal order.

The Problem of Unwarranted Generality

When a case study is presented in support of a principle or generality, it is the duty of the textbook writer to specify whether the material is used simply to illustrate a particular feature of an area, model, or theory; whether it is exemplary of patterns found elsewhere; or whether its status in regard to the latter is unknown. Without such statements or qualifications the reasons for citing the empirical example remain unclear and, worse perhaps, unwarranted inferences may be drawn. Among the most pernicious of these is the conclusion that the example is representative of the situation elsewhere. One might argue that there is little difference between an error of omission that may lead to an incorrect conclusion and the situation in which an author has explicitly claimed generality but is wrong.

To return to the Szechwan market patterns, Skinner noted that the area under examination was characterized by an "exceptional . . . human ecology." More specifically, he stated that in the Szechwan Basin, "neither nucleated villages nor minor markets obtain" (G.W. Skinner 1964:6). Peasants in Szechwan lived in farmhouse clusters and dispersed farmhouses, in contrast to the more common pattern of nucleated villages. Although Skinner downplayed the differences between the study area and other regions of China, the germane point is that his findings may or may not be the norm for China as a whole. This is an empirical question, not a matter of informed guessing.

When one examines the literature on periodic and daily markets, it is clear that others have found neither hexagonal market areas nor those which approximate the hexagonal (Symanski 1971; Bromley 1974; R.H.T. Smith 1978). Furthermore, there are various types of marketing systems that do not fit classical central place patterns along a number of dimensions. These other types include network systems, dendritic systems, and those which are solar, the latter being something of a commonplace in the Third World (C.A. Smith 1976:1,36).

Ironically, there is one major exception to the generalization that Skinner's findings are unique; it is the result of Carol Smith's research in highland Guatemala (C.A. Smith 1972). She performs transformations very similar to Skinner. This is not surprising; she was his student. Nor is it surprising that Skinner's and Smith's work should have been disseminated to the larger scientific community, with a heavy emphasis on the hexagonal transformations, by another student of periodic markets, an anthropologist who was both friend and colleague to both Skinner and Smith at Stanford University (Plattner 1975). Drawing on the market material from China and Guatemala, Plattner claimed in *Scientific American* that they provide evidence that a theory "built on several

formal simplifying assumptions that are so abstract as to seem entirely unrelated to the real world . . . [should be] . . . shown to be strikingly appropriate to several real world agrarian societies" (Plattner 1975:66). At the very least, Plattner is misleading; less generously, he is wrong.

Textbooks in geography present a rather uniform pattern on this issue. For example, in Kolars and Nystuen's *Human Geography*, there is no mention as to whether China's hexagonal market areas are the norm, or what kinds of patterns are found elsewhere (Kolars and Nystuen 1974:93-97). After discussing the China case, the authors move on to Iowa, an area where the topography and history of settlement stack the deck in favor of the theoretical norm. If there is any sense in which these authors take care in drawing attention to the uniqueness of their material, it is in the somewhat oblique way of noting that the Skinnerian hexagons are a condition of spatial equilibrium, reached after a long period of stability in Imperial China (Kolars and Nystuen 1974:93). At chapter's end they also note the possibility of departures from theory by drawing on a single example from Ontario (Kolars and Nystuen 1974:106-109).

In a more recent text on economic geography, Berry *et al.* reproduce four of Skinner's maps and cartograms, two in the context of the K = 3 central place pattern, and two for the K = 4 model (Berry *et al.* 1976:232-34). To these authors, the Skinnerian example is "outstanding," indeed, "startling" (Berry *et al.* 1976:232, 234). In context, there are no qualifications or statements about the generality of the findings, even though at least one of the authors — Berry — is surely aware that such patterns are not found elsewhere in the Third World. Some five pages later, Berry *et al.* do state that "Skinner's description of the periodic marketing system of traditional rural China may be used as an example, although details vary from one part of the world to another" (Berry *et al.* 1976:239). Although one might take this caveat to be a qualification to all of the Skinner material presented, the statement shows up not in a discussion of trade area shapes, but rather in the context of periodic market calendars and vendor mobility, related and yet distinct phenomena.

Four other textbook writers are no more attentive to the question of generality. It would presumably be argued that Lloyd and Dicken can be excused on the grounds that their book, *Location in Space*, is subtitled, *A Theoretical Approach to Economic Geography* (Lloyd and Dicken 1972), while, to complete the ring of circularity, one might argue the same for English's treatment in *A Question of Place*, subtitled, *World Regional Geography*, since the hexagon transformations are included in a chapter on China (English 1977:70). No matter, of course, that the question of generality for China is, as mentioned previously, itself problematic, or that English, in discussing marketing areas, makes the erroneous generalization that "this economic structure of traditional China is an example of the regularity and rhythm in the human economy apparent in many preindustrial societies" (English 1977:71). Finally, Peter Haggett includes the Skinnerian diagrams in the first and second editions of his *Geography: A Modern Synthesis* (1972:293-94; 1975:371-72). Again, there is no mention of how representative the case is, and patterns from elsewhere are not cited.

Replication has not been possible. The area studied by Skinner is remote in space and time, China has been effectively "closed" to researchers who might have wanted to check the findings, and — at any rate — checking documentary research in a difficult foreign language would have precluded careful re-analysis by virtually all of those with relevant theoretical and methodological tools. Without so much, then, as a single contradictory test of Skinner's "remarkable" results, combined with their exotic setting,

it is little wonder that textbook writers have been so eager to reproduce his visual claims.

Has any skepticism been voiced in regard to the specificity or universality of Skinner's results? Apparently not much, at least as recorded in the literature. One has to search hard for a statement such as Ginsburg's that "he was less inclined than Murphey [a distinguished geographer whose main interest is China] to describe Skinner's work . . . as being exemplary, especially given its regional specificity within China" (Ginsburg 1973:274-75). Ginsburg, a longtime student of Asia and of development issues, also voiced skepticism that Skinner's findings were a "reliable guide to recent or future trends in the organization of area in the developing countries" (Ginsburg 1973:275).

A still further issue, mentioned in none of the textbooks, concerns the very testability of the theory that generates the hexagons. If central place theory cannot be tested, then it might be asked just what Skinner has done. Or, more appropriately perhaps, what authors think he has done. In this regard, we can do no better than quote from Webber who has addressed himself to the empirical verifiability of central place theory:

> *Now it is clear that, however difficult may be the task, any study which sets out to validate or disconfirm central place theory by examining whether its predictions hold in reality, must attempt to control . . . environmental assumptions. . . . It is equally obvious that no study has held these conditions constant, not even those studies of the near-homogeneous state of Iowa. It follows that* the predictions of central place theory have never been empirically tested, *and that the theory has been neither confirmed nor disconfirmed" (Webber 1971:27; our emphasis).*

The problems we have pointed to in this extended examination of a single piece of research derive, in good part, from too much concern for order and too little interest in skepticism and criticism. Those who have written the textbooks cited should, as a matter of principle, have been more cautious in presenting Skinner's findings. Yet they bear only a portion of the blame. Those whose primary research interest is periodic markets now show little concern for whether or not hexagonal market areas exist — in company with the great majority of location theorists. Nevertheless, market researchers have been no more skeptical of Skinner's well-known and often-cited hexagons than have outsiders.

One of the more unfortunate outcomes of the relentless search for spatial order is the way apparently good, "soft" information is overlooked or ignored. Whatever the nature of the shortcomings of Skinner's attempt to demonstrate hexagonal order — and they appear to be considerable from the standpoint of social science methodology — the rich historical detail he provides on China's internal marketing systems deserves attention it clearly has not received. In all probability, of course, anything he had to say about the spatial organization of Chinese society would have been ignored had he not put some of his claims into an attractive cartographic and geometric format. Skinner is not responsible, however, if others have used his work to buttress the dogma that the world is highly ordered. And textbooks perpetuate dogma as well as any medium.

5

The Case of the S-Shaped Curve

If one source of uncertain order is the uncritical acceptance of exotic findings based on unreported data and unjustified "transformations" in a cultural setting where checks and replications cannot be made, another problematic area is the nature of methodology itself. In particular, it is the positivist practice of implying that methodology *is* theory, that there are no grounds other than methodology for questioning findings. A good example is seen in the study of the diffusion of innovations where, among geographers and social scientists alike, in both textbooks and research designs, methodology has been seen as sufficient for explanation of spatial and temporal order (Abler *et al.* 1971; Hägerstrand 1952, 1967; Rogers 1962; Mansfield 1963; Casetti 1969; Casetti and Semple 1969; I. Feller and Menzel 1978; Hamblin *et al.* 1973; Hamblin and Miller 1976). The problem has not been attenuated by recent work that questions the empirical adequacy and theoretical relevance of this kind of diffusion research (Brown and Cox 1971; Downs and Mohr 1975).

The field of diffusion studies has been dominated by three "principles of order": the logistic or S-curve for diffusion in a temporal context, the neighborhood effect (contagion) for diffusion in a spatial context, and the hierarchy or short-circuit effect for diffusion in the context of a central-place system. Much diffusion research has, until recently, focused on the verification of, or search for, these "empirical regularities." Although there is some controversy over whether these regularities fit together very well and each has had its critics, they have become permanent fixtures of research designs and textbooks. In this chapter we focus on the reasoning involved in using the method of curve-fitting as a means of *explaining* the process of diffusion over time.

Innovation Diffusion and Time

The approach in question typically postulates a diffusion process that produces an S-shaped (sigmoid or logistic) curve of innovation adoption over time and then fits actual adoption data to this in order to *explain* process. That is, the reasoning involves an *a priori* postulation of process.

In practice, the strategy involves selecting one of the following processes upon which an S-curve model can be based. (1) The time of adoption of any given innovation "is determined by the interplay of an infinitely large number of elements in the social milieu" of the potential adopter, thereby fitting the requirement of the normal distribution

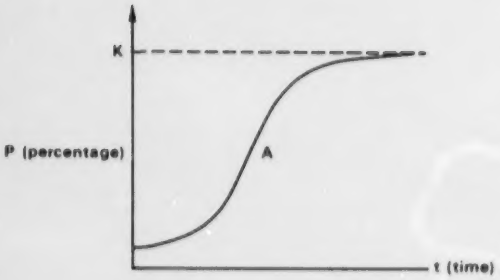

FIGURE 3 THE LOGISTIC GROWTH CURVE

that "the value of each event is the result of a chance combination of a great many minute and relatively equal factors" (Pemberton, 1936:550, 549). (2) The cumulative normal curve is the same as an individual's learning curve in behavioristic psychology. Adoption is then equivalent to a learning trial (Rogers, 1962:153). (3) There is an interaction effect — adopters influence those who have not adopted. As more people adopt, the effect on non-adopters increases (Rogers, 1962:154-155). In the geographical literature this third process is usually held to underlie patterns of innovation adoption. Rather than the cumulative normal distribution, which assumes many random acts in a temporal series, a logistic growth curve better meets the needs of an interaction process because it assumes a set of "one random steady pair" acts (Dodd 1955:397). Curiously, while some geographers assume an interaction effect, they have derived their S-shaped curves from normal distributions (Hägerstrand 1967; P.R. Gould 1975). One example is Amedeo and Golledge (1975:272) who state that:

> *If we begin with the normal distribution of innovation acceptances and transform this distribution into one that shows the cumulative percent of people adopting over time, the result is a diffusion curve which takes an S-shaped form. This generally has been described as a logistic curve.*

Curve-Fitting and Explanation

Many studies have suggested that the diffusion of innovations conforms to S-shaped time trends (Rogers and Shoemaker 1971). It was from this empirical work and at the suggestion of certain sociologists, especially Tarde (1903) and Chapin (1928), that curve-fitting models became favored conceptual devices (Hamblin *et al.* 1973). Some researchers, particularly economists, viewed these models in a descriptive light, as a first step in producing an explanation. For instance, Griliches used logistic curves to summarize his data, obtain parameters, and investigate the parameters using exogeneous variables (Griliches 1957:502). Our argument is not concerned with such legitimate uses, but rather with research using curve-fitting as a theoretical device.

For the self-conscious instrumentalist, prediction and explanation are symmetrical. Prediction is the same as an explanation made *before* the event, an explanation the same as a prediction made *after* the event. A theory, such as the social interaction theory of innovative diffusion, is judged solely by its predictive value and not by its explanatory truthfulness. In diffusion research, this subtle distinction has been overlooked. Curve-fitting has been seen as a means for testing the truthfulness of theory as well as a predictive device for evaluating the usefulness of a statistical model.

Pemberton (1936) was one of the early users of S-shaped curves as a test of theory. In geography, Hägerstrand (1953) was the first to take up this approach. As Amedeo and Golledge (1975:253-254) point out, Hägerstrand's models that simulate the spread of an innovation through an area are consistent with the model that describes the change in the number of adopters per unit change in time, "the S-shaped growth curve." Over the past twenty years many geographers have followed in Hägerstrand's footsteps as researchers and/or proselytizers (Casetti and Semple 1969; L.W. Bowden 1965; P.R. Gould 1975).

The S-shaped curve is usually specified as logistic; mathematically it takes the following form:

$$P = \frac{K}{1 + e^{-(a + bt)}}$$

where, for diffusion of innovation studies, **P** is the percentage of potential adopters who have adopted the innovation, **K** is the asymptotic ceiling value of **P**, **t** is time, **a** is a constant of integration locating the curve on the time scale, and **b** is the coefficient for rate of growth. Adoption of the innovation by potential users increases at an increasing rate throughout the first half of the diffusion process, reaches an inflection point, then increases at a decreasing rate to the asymptotic ceiling (Figure 3).

Underlying the logistic growth model is a theory of innovation diffusion that rests upon a process of interaction in which adopters "persuade" non-adopters to adopt. To "test" the theory, empirical data are fitted to the logistic growth curve. Thus, curve-fitting becomes an explanatory, rather than a descriptive or predictive, device. Pemberton (1936:548) provided the following argument for this approach:

> *The mathematical equation used should be constructed on the basis of the specific theory of culture interaction which could be considered the most acceptable explanation of the curve of culture diffusion. That is to say, the a priori conditions of the mathematical equation should have their theoretical counterpart in the conditions of culture interaction producing each sequence of diffusion.*

Hägerstrand (1967:274) supported this perspective strongly when he reported on the fit between a growth model and an empirical curve:

> *The two growth curves correspond quite well with one another. It should be remembered that the theoretical curve is not a fitted mathematical function which was sought out from among those resembling the empirical curve, but rather that it is produced as a consequence of the stated assumptions.* The degree of correspondence ought to indicate that these assumptions are essentially sound [*emphasis added*].

Others have echoed agreement. Casetti (1969:101-102) writes that:

> *The conformity of the diffusion of innovations to doubly asymptotic trend curves generally has been regarded as a finding of a formal nature, useful at most for summarizing data and for identifying certain parameters of the diffusion process, such as its positioning in time and its rate of progress. This paper attempts to link a class of* models of diffusion mechanisms *to a class of S-shaped trend curves and to formalize some aspects of Hägerstrand's work [emphasis added].*

It has also been proposed that the logistic model of diffusion be seen as a "synthetic theory" in which one begins with certain postulates relating the basic properties of the theory and then generates deductions from them (Coleman, 1964:41-46). The logistic curve is thus an empirical representation of a theory which is, at a "deeper" level, represented by a logistic function. Coleman, however, sees the logistic as only *one* among several plausible "diffusion theories" focused on different mathematical functions.

These statements require, of course, that a "theory of culture interaction" and diffusion is readily identified with an S-shaped curve (logistic or otherwise). The corollary also must hold. The S-shaped curve should be readily identified with a specific theory. It is on this point, as we shall see later in detail, that curve-fitting as a mode of explanation breaks down. It breaks down because we cannot unambiguously associate a specific theory with a particular pattern in the data. Theories *other* than the one that has been specified may also account for the pattern (W. Feller 1966:52).

The Confusion of Instrumentalism and Realism

The many "good fits" obtained between ideal and actual curves have lead some researchers to make extreme claims on behalf of the theoretical value of their exercises (Hamblin *et al.* 1973). In particular, accumulation of empirical evidence has led to the sanctification of the logistic as *the* model of diffusion (Casetti 1969). Why should there be such an intellectual gap between these proponents and those who might stress the limitations of curve-fitting as a mode of explanation? We believe that there are four grounds for this.

First, there is a confusion between *tautological* and *functional explanation.* The first rule for any scientific hypothesis ought to be that it is at least possible to conceive of an observation that would contradict the theory. What good is a theorem that is guaranteed by its internal logical structure to agree with all conceivable observations, irrespective of the *real* structure of the world? Yet is this not exactly the situation with fitting logistic growth curves to diffusion data? Within a closed social system defined by an asymptotic ceiling, it is unclear how one could arrive at a growth curve which is *not* dampened at either end by the limiting conditions of few initial adopters and a limited total population. Though the shape and height of curves might differ across innovations, their essential S-shape would be maintained. In effect the S-shaped curve is an inevitable concomitant of the conditions that define it and, hence, a self-fulfilling prophecy.

A second factor involves the *kinds of innovation* about which information has been collected. Downs and Mohr (1975:54) note:

> *We believe that the ubiquitousness of S-shaped diffusion curves is partially an artifact of the kinds of innovations that are usually studied. For the most part, these have consisted of fairly unambiguous technological advances which eventually diffused to most of the population.*

This "selection bias" has further reinforced the self-fulfilling nature of fitting S-shaped curves and institutionalized the "lawfulness" of the logistic. The constants of the logistic curve have thus been interpreted as constants of nature, rather than specific to innovations and settings.

Third, the advocates of fitting S-shaped curves have adopted a *social interaction* and *communications* theory of diffusion that relates neatly to their *methodological imperative*. This underlying rationale has prevented consideration of a whole range of phenomena that affect the diffusion of innovations. Other writers critical of curve-fitting as a satisfactory mode of explanation have stressed innovation support and sponsorship by economic and political institutions (L.A. Brown 1975; Blaut 1977; Yapa 1977; Blaikie 1978), innovation attributes (Perry and Kraemer 1978), and innovation origin and early promotion (L.A. Brown 1975; Warner 1974). In this connection Downs and Mohr (1975:46) offer the following observation:

> *Diffusion curves may strongly suggest that communications-related variables are important for innovation, but they do not demonstrate the importance, nor do they quantify it,* especially in relation to causes of other types *(emphasis added).*

Finally, those who use S-shaped curves for explanatory purposes are limited by the *essential instrumentalism of curve-fitting* into restricting their investigations to patterns in the data and *inferring* process from pattern. Yet finding a pattern and inferring a process are by no means as self-evident and straightforward as many advocates of curve-fitting imply (Hamblin *et al.* 1973; Amedeo and Golledge 1975). In the first place, curve-fitting is an indeterminate procedure (Harré 1972). If, in Figure 4, each curve represents a pattern-process combination (or "law") inferred inductively from the premises represented by the data-points, and from each curve the points ("the facts") can be inferred, which of the pattern-process combinations represented by the curves is the correct one? We could say that goodness-of-fit is the most important criterion. Alternatively, by invoking the Principle of Simplicity, we could argue for the superiority of the "simplest" (Harré 1972). Which way should we go? Both **A** and **B** in Figure 4 are visually satisfactory. Further data collection (or experimentation) would not reduce the ambiguity with respect to making a choice between **A** and **B**. We need some basis for making a judgment. Given the curve-fitter's restriction to the observed facts we can do no better than arbitrarily assume that the simplest is the best. However, as Mario Bunge (1963) points out, "simplicity is not that simple." In particular, simplicity in interpretation may conflict with notational simplicity (parsimony in mathematics). In Figure 4 interpretive simplicity might lead us to accept curve **A**, whereas mathematical simplicity might lead us to prefer curve **B**. Further, as Harré (1972:45) notes, "the history of science can offer little comfort to the believers in simplicity. . . . There can be no doubt that the laws of nature are always more complex than we originally thought." He might have added, the "laws" of society too.

This leads to a second point. Accept curve **A**. On what basis do we infer that this curve (which is something akin to a logistic) is accounted for by a specific theory of social interaction? In truth, there is no basis at all. Curve **A** (and its associated algebraic representation) could follow deductively from a number of theories. Of course, instrumentalists have little interest in this point. Their concern with theory as an "inference ticket" means that the order found by way of curve-fitting (and the Principle of Simplicity) requires no special pleading, and no further work. Anyone who attempts to

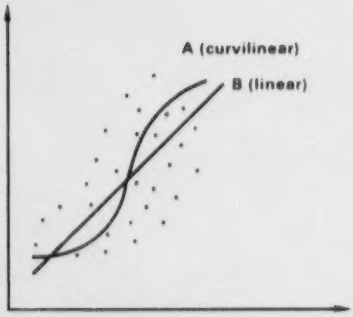

FIGURE 4 THE INDETERMINACY OF CURVE-FITTING

begin with pattern and infer process, however, is faced with the inherent difficulty of knowing that he has identified the right process(es). The idea of "equifinality" tells us that because different processes can produce similar patterns the recurrence of a specific pattern does *not* indicate the recurrence of a specific process (Olsson 1969).

Curve-fitting is a simplistic, if "successful," way of searching for and claiming temporal order in the diffusion of innovations (Agnew 1979). As a means of systematizing data in an instrumentalist manner, it can be defended as a useful descriptive device. Further than this one cannot go. As a predictive mechanism, fitting the logistic curve has been subject to considerable criticism (Bowley 1925; Wilson and Puffer 1933; Dorn 1950). When an instrumentalist model of scientific practice (such as curve-fitting) is combined with realist (truth-finding) intent, the result is even more intellectually disastrous. Unfortunately, many diffusion researchers are clearly not aware of the theoretical limitations of their preferred methodology and the negative effects it has had on the growth of diffusion theory (Hägerstrand 1967; Casetti 1969; Hamblin *et al.* 1973; Gray 1973).

Seek and Ye Shall Find

Earlier we argued that when a principle of order becomes well-established there is a tendency to keep "discovering" it unless there exists some countervailing mechanism that facilitates skeptical questioning. Pressures come from various directions: traditions of data collection and manipulation, the demands of clarity in professional and classroom presentations requiring order rather than disorder, and the imperative to *explain* rather than merely *describe*. Whatever the specific pressures in operation, the "self-fulfilling prophecy" will be a likely outcome. The confidence that many diffusion researchers have placed in the repeated appearance and, hence, the theoretical value, of S-shaped curves is illustrative of this process. An S-shaped curve is assumed, a curve is fitted to data to show the existence of one, and it is argued that an S-shaped curve represents the discovered process! This method of reasoning picks up momentum and takes on a life of its own, not just because of the number of times it is repeated, not just

because no attempt is made to uncover *real* processes, not just because the outcome is ordained by the assumptions, but also because other possibilities besides an S-shaped growth curve have not been specified. The approach taken in textbooks (Haggett 1975; Berry *et al.* 1976), a programmed learning package (Hamblin and Miller 1976), and some recent research articles (Gray 1973; I. Feller and Menzel 1978) shows this reasoning in operation.

In two widely circulated textbooks (Haggett 1972; and Berry *et al.* 1976), the S-shaped curve is introduced to students as a major empirical regularity in the diffusion of innovations. In his chapter on "Spatial Diffusion," Haggett argues, in a vein similar to that of many other textbook writers (Cox 1972; Lloyd and Dicken 1977), that "resistance of a population to adopting an innovation usually follows an S-shaped curve." He then proceeds to present an exposition of how to construct a logistic curve (Haggett 1975:358). No mention is made of the *special* conditions under which such a curve makes conceptual sense, in particular the assumptions of homogeneous mixing within a finite population and an adoption rate directly proportional to the populations of adopters and non-adopters. Moreover, there is no statement of the apparent *inconsistency* between the logistic form of the S-shaped curve and the neighborhood effect noted by Brown and Cox (1971:554) and Hudson (1969a:45-58; see also Walker 1973:1187). Clearly, the former "principle of order" implies spatially homogeneous mixing, whereas the latter requires a distance-biased contact pattern. Finally, information diffusion is erroneously equated with innovation diffusion. Indeed, the conditions under which innovations are supplied to a population are not treated at all. Hearing is equivalent to adopting in Haggett's exposition. This emphasis on information diffusion is not unusual among textbook writers, however. Most presentations on the diffusion of innovations ignore the supply of innovations and focus on the spread of information about innovations (Cox 1972; Amedeo and Golledge 1975; Lloyd and Dicken 1977).

For Berry *et al.* (1976) the S-shaped curve of the logistic distribution is a transcendental "law of growth." The authors stress the isomorphic nature of logistic curves throughout the natural and social sciences. In so doing they call to mind the discussion of "isomorphic laws" initiated by the founder of General Systems Theory, von Bertalanffy (1950, 1956). The logistic model of innovation diffusion can thus be regarded as one of geography's contributions to the construction of a "unified" science such as that envisaged by von Bertalanffy. However, the process underlying the logistic model in a diffusion context and its relevance for understanding the diffusion of specific innovations remain unexamined.

In the attempt to illustrate that "we too fit into the sciences," Berry *et al.* do not discuss evidence which suggests that the logistic curve is far from a transcendental law of growth, even without geography's contribution; that, indeed, it may even be tautological and theoretically misleading. Since the 1920s, especially in demography and genetics, substantial agreement exists that fitting logistic curves is of limited utility as either an explanatory or predictive device. For instance, decades ago in biology W. Feller (1940:52) argued that:

> *. . . at the present stage, the recorded agreement between the logistic and actually observed phenomena of growth does not produce any significant new evidence in support of the logistic, beyond the great plausibility of its deduction. Nor is the closeness of the agreement as strong as it appears at first sight since equally good results can be obtained by applying other mathematical forms, corresponding to quite different biological assumptions.*

This observation has not been refuted. Indeed, not long ago, a British demographer, referring to the time in the 1920s when the notion of a logistic "law" of population growth was in vogue, noted that "much of the confidence . . . that general laws for describing population change could be discovered and formulated in precise quantitative terms has gone" (Brass 1974:532). Berry *et al.* (1976) and others (Hamblin *et al.* 1973) do not share this breakdown of confidence. Indeed, Hamblin *et al.* refer to the work of those who proposed the logistic law of population growth in the 1920s as indicative of the strong intellectual base in the social sciences upon which their curve-fitting approach rests.

An extreme commitment to the logistic curve also characterizes a programmed learning package by Hamblin and Miller entitled "Introduction to Mathematical Patterns of Cultural Diffusion" (1976). The authors state that "the major purpose of this learning package is to help the student understand the process of cultural diffusion, that is, how new elements (whether borrowed from another culture or invented within) spread among a population of people or other units" (Hamblin and Miller 1976:4). Apart from the "special case" when "outside agents" are responsible for the spread of information, the logistic model is portrayed as a sufficient basis for understanding cultural diffusion. The authors allow only for "methodological" error:

> *One value of a mathematical equation is it allows the human mind to organize a large amount of information. Yet an equation will never fit real-world data perfectly because of measurement and other types of error (Hamblin and Miller 1976:21).*

The main trouble with this, of course, is that it ignores the problem of conceptual error. In particular, not only the logistic distribution but the normal (which the authors also use but do not distinguish from the logistic), the Cauchy, and many other distributions having quite different bases, can be fitted to diffusion of innovation data with the same or sometimes better goodness-of-fit (see, for example Bowley 1925; Rapoport 1953; Coleman 1964; Brown 1968; Nabseth and Ray 1974). Estimation of fit is more difficult with these other models, however (Brown and Cox 1971:552). One suspects that at least part of the logistic's attraction lies in its mathematical and estimation advantages rather than its theoretical appropriateness.

Other social scientists exhibit attachment to S-shaped curves by explicitly basing their research on the perspective expressed by Pemberton (1936) and Hägerstrand (1967). Recent articles by Gray (1973) and I. Feller and Menzel (1978) utilize logistic-based S-shaped curves to examine the diffusion of policy and policy-related innovations among state and municipal governments. Although more traditional research focuses on diffusion as the outcome of interaction between *individual* adopters and non-adopters, Gray argues that non-adopter *governments* emulate adopter governments. In order to use the logistic model, her approach involves the assumption that state governments constitute "a completely intermixed population" (Gray, 1973:1179). Yet, as Walker (1973:1188) has pointed out:

> *Gray certainly recognizes that regional biases in the flow of communications among adopters may make it unreasonable to regard the states as a completely intermixed population and refers several times to the possibility of creating more elaborate models of diffusion by relaxing this fundamental assumption,* but presumably this would be a much more

> difficult undertaking that would not have the economy or simplicity of her present model [*emphasis added*].

In their research, I. Feller and Menzel (1978) claim good fits between data and logistic growth curves. But it is unclear, given the content of their paper, why discussion of their data needed reference to the logistic function except in deference to "orthodox practice." Certainly, there is no self-evident argument as to why the diffusion of municipal innovations *should* conform to logistic trends. Feller and Menzel definitely present none. Other research (Eyestone 1977; Perry and Kraemer 1978; Agnew *et al.* 1978) *and Feller and Menzel's own efforts* suggest that many policy and technological innovations diffuse from a single source (the federal government) and that innovation attributes and local government needs are more important for diffusion than interaction between adopters and non-adopters. Yet the theory which the S-shaped curve (logistic) is held to represent in a diffusion context is focused on interaction between adopters and non-adopters as the *sole* causal mechanism of adoption. All things considered, therefore, the logistic model *and* its theoretical basis appear to be of limited usefulness in predicting and substantively explaining the diffusion of policy-related or other innovations.

Statistical Regularity and Cause

For science, statistical regularity is but a first step in the long process of investigation that ends only when the reasons for the statistical regularity have been identified. As argued in this chapter, apparent order in a statistical series is not the same as proclaiming order in "causal" relations. Moreover, the seeming "lawfulness" of the logistic (and other functions that generate S-shaped curves) has seduced researchers of the diffusion of innovations into believing that mathematical functions can have explanatory power *beyond* any specific empirical case. Statistical constants have been seen as *natural* constants. Out of this perception has stemmed a resistance to treat different innovations as potentially having different diffusion processes associated with them or, logically, that specific innovations do not diffuse at all, but are adopted simultaneously. The ordering dogma dictates, and the textbooks pass on the "good news," that the spread and adoption of innovations over time conforms, in a rather simple way, to a sigmoid or logistic curve.

This case study illustrates how a remarkable mix of instrumentalism and realism has led researchers and textbook writers alike into proclaiming that curve-fitting has both predictive and explanatory significance. The amalgamation of an instrumentalist methodology with a realist intent, however, produces no such outcomes. Rather, it has given rise to theoretical stagnation in the form of a sanctified theory of primitive social interaction ("knowers" telling "non-knowers"), and an obsession with the marvelous order *revealed* by application of a curve-fitting technique. The explosion of "scientific reasoning" in geography in the 1960s has led us far from Popper's (1962:228-9) argument that:

> *The rationality of science lies not in its habit of appealing to empirical evidence in support of its dogmas — astrologers do so too — but solely in the* critical approach *— in an attitude which, of course, involves the critical use, among other arguments, of empirical evidence (especially in refutations).*

6

The Case of the Rank-Size Rule

The process of "scientific reasoning" described in previous chapters has had widespread and persistent negative effects within contemporary human geography. In this chapter the persistence of the approach, despite its failings, is indicated by a discussion of the writings of geographers on the size-distribution of cities. In this context the approach has led to the canonization of the "rank-size rule" as one of urban geography's major "law-like" statements. In this case we show that attachment to the rank-size rule, as with market area hexagons and S-shaped curves, is a product of commitment to order in the absence of a skeptical intellectual posture. More specifically, three issues raised in the previous chapter, the problem of equifinality, reliance on the *Principle of Simplicity* and the tendency towards a self-fulfilling prophecy, are again relevant.

The Rank-Size Rule

Some empirical research suggests that city-size distributions are positively skewed; that is, there are many small but only a few very large cities, with a tendency for the number of cities in each of a set of size classes to decline as city size increases. Lognormal, Pareto, and rank-size distributions offer the possibility of mathematical fit to such empirical distributions; all three have close similarities (Richardson 1973). The lognormal is the simplest and offers a good fit if the frequency distribution of city sizes is humped and positively skewed. According to Richardson (1973), it is given by:

$$\mathbf{N} = \log \mathbf{P} \qquad (1)$$

where **N** = cumulative percentage of cities and **P** = city size. The lognormal covers the whole range of city sizes, and this differentiates it from the Pareto distribution which deals only with the upper tail (city sizes above a defined threshold level, $\overline{\mathbf{P}}$). The Pareto is given by:

$$\mathbf{N}(\overline{\mathbf{P}}) = \mathbf{A}\mathbf{P}^{-\alpha} \qquad (2)$$

where $\mathbf{N}(\overline{\mathbf{P}})$ = cumulative percentage of cities above the threshold level, $\overline{\mathbf{P}}$; **A** and α = constants. This can be represented as:

$$\log \mathbf{N}(\bar{\mathbf{P}}) = \log \mathbf{A} - \alpha \log \mathbf{P} \tag{3}$$

which is similar to (1).

The rank-size distribution is identical to the Pareto except that rank of city is used instead of cumulative percentage of number of cities, and it covers all cities. It is as follows:

$$\mathbf{R} \cdot \mathbf{p}^{\mathbf{q}} = \mathbf{K} \tag{4}$$

where **R** = city rank; **q** and **K** = constants. This can be rearranged to yield:

$$\mathbf{R} = \mathbf{K} \cdot \mathbf{P}^{-\mathbf{q}} \tag{5}$$

Which is the usual way of representing the rank-size distribution. A special case of the rank-size distribution is obtained where **q** = 1. This is the so-called rank-size rule:

$$\mathbf{R} \cdot \mathbf{P} = \mathbf{K} = \mathbf{P}_1 \tag{6}$$

where $\mathbf{P}_1$ = the size of the largest city. Consequently, the product of city size and its rank is a constant, equal to the size of the largest city in the set of cities being examined. In much geographical writing the concept of the rank-size rule is loosely associated with *all* of the functions described here, though only one is *properly* described as such (Robson 1973).

Empirical discovery of the rank-size distribution of cities in a nation or "economic" region is usually attributed to Auerbach (1913). Lotka (1924) and Singer (1936) corroborated Auerbach's findings. However, these efforts did not attempt to propose a theory explaining the rank-size distribution or the more specific rule. Only with Zipf (1941, 1949) is there a proposal to explain the rank-size distribution in terms of a "rank-size theory." Since then several other theories have been proposed. These include attempts to incorporate the mathematical functions of city-size distributions into central place theory (Beckmann 1958), economic growth theory (Davis and Swanson 1972) or, most typically, to model city-size distributions as probabilistically derived steady state equilibria (Simon 1955). Currently, it does not seem possible to favor any one theory over the others on the basis of its predictions since, as Richardson (1973:239) argues, "any one of the observed statistical relationships is compatible with several theories while many of the individual theories are consistent with more than one of the standard empirical distributions." To overcome this impasse, Richardson advocates evaluation of the hypotheses and assumptions that underpin the theories, rather than the orthodox fitting of predicted and actual city size distributions (also see Johnston 1977; London 1979; Miller 1980). It is with this orthodox procedure, however, that the rest of this chapter is concerned. We begin by examining that work which has been most influential in developing "rank-size" theories.

Zipf investigated at great length the mathematical properties of rank-size curves. In particular, he examined the rank-size curves of cities (by population), biological genera (by number of species), books (by number of pages), and many other phenomena. Zipf found that in most cases the curves were approximately segments of rectangular hyperbolas. When such curves are plotted on log-log paper they are straight lines with slopes close to -1. He claimed to have discovered a "general law" of the form **rank** x **size** = **constant.**

Having established this statistical regularity, Zipf endeavored to *derive* his law from theoretical considerations, which he summarized in his so-called "principle of least effort." According to this principle an action is governed by an organism's drive to solve both immediate and perceived future problems so as to minimize the total work involved. In the context of the size-distribution of cities, Zipf argued that owing to the dual role of the individual as both consumer and producer a conflict arises in the *economy of location*. To minimize effort as a producer the individual should live close to where he is employed, while to minimize effort as a consumer he should live adjacent to the points of consumption. The contradiction between these roles resolves itself, at a higher scale, into a conflict between forces of "diversification" and "unification." The force of diversification, acting to minimize effort in the producer role, leads to the location of population in proximity to scattered sources of raw materials, whereas the force of unification, operating to move materials and production to the population, gives rise to a settlement pattern in which there is a concentration of population in fewer but larger cities.

Zipf used similar arguments for the other substantive phenomena that he found had rank-size distributions. In general, however, his efforts met with a cool reception. The problem was Zipf's insistence that rank-size distributions provided *evidence* for the operation of his "principle of least effort." As Rapoport (1968:320) notes:

> *Similar distributions may be traceable to mathematically isomorphic processes, but Zipf, in his principle of least effort, emphasizes not the possible mathematical genesis of his law but its alleged origins in the nature of human behavior.*

Rather than such a questionable association of substantive explanation and statistical regularity (the equifinality problem), Rapoport suggests that a statistical rationale, based upon the probabilistic structure of events, can be used to account for the frequency density and, hence, the rank-size distributions. It is the statistical structure of events rather than their content which explains their distribution.

Herbert Simon (1955) was the first to provide a probabilistic or "stochastic" rationale for rank-size distributions. He argued that the distribution of city sizes, for example, can be regarded as the steady-state equilibrium of a stochastic process, and approximately described by a Pareto distribution. The specific stochastic growth process is the Law of Proportionate Effect (Gibrat's Law) in which a set of entities — in this case, cities — grow in such a way that the rates of growth are independent of the initial sizes of the entities. For a given time period, growth rates vary randomly in each size class around an average growth that is the same for *all* size classes. Over time, this process produces a Pareto distribution (Simon 1955). Simon suggested that "demographic forces" might plausibly *explain* the operation of the "law." He was careful to note, however, that one cannot assume such an explanation to be consistently operative wherever highly positively-skewed, J-shaped frequency distributions occur (Simon 1955:440).

According to Simon, it is in the "weakness" of his statistical model that its strength lies. For although there is a weak "behavioral theory" implicit in the model, it is one that presupposes little more than a minimum of constraints on *chance*. The skew distributions noticed by Zipf and analyzed by Simon are seen as the results of the *same kind* of stochastic process. Hence, for Simon, it is unreasonable to suppose the operation of an underlying teleological principle, such as the "principle of least effort," especially since the results can be derived from simpler assumptions.

Geographers and the Rank-Size Rule

Simon's stochastic model of rank-size distributions entered the geographical literature on city-size distributions in an article by Berry and Garrison in 1958. Although they recognized the general nature of the assumptions and lack of explicit attention to substantive explanation inherent in Simon's model as major drawbacks, Berry and Garrison nevertheless opted to accept the rank-size rule as ". . . an average condition, both from empirical and theoretical points of view" (Berry and Garrison 1958:90). The term "theoretical" is of some importance. Its meaning in this context derives from the "steady-state" nature of the rank-size rule as predicted by Simon's model, which enables empirical exceptions to be explained away in terms of stochastic variation around the norm. Moreover, and with respect to the findings of empirical research on city-size distributions in several regions, Berry and Garrison (1958:90-91) argued that:

> *Absence of significant differences between frequencies would indicate that similar processes of urbanization were operating in both regions; presence of significant differences would indicate the operation of differential forces of urbanization.*

Noting the cautions of Simon, Berry and Garrison essentially *abandoned* his model in search of substantive forces masked by the stochastic nature of the model. Within the context of Simon's model, there is no way to evaluate whether similar processes of urbanization are indeed operating in two real-world regions. Simon clearly noted that identical empirical distributions may be produced by different and sometimes contradictory generative processes (the equifinality problem, again). This, of course, is the same problem we encountered with S-shaped curves in the previous chapter: curve-fitting is seen as having *substantive* theoretical value when it does not. Berry and Garrison have, so to speak, turned from a stochastic to a substantive mode of reasoning without noting what they have done. Furthermore, the acceptance of the rank-size distribution as an empirical "average condition" is quite misleading. Indeed, *many* countries have city-size distributions considerably different from the skew distribution of the rank-size rule.

In a 1961 paper Berry moved toward a "disaggregation" of the stochastic model into a set of "causal forces." After investigation of the city size distribution of thirty-eight countries differentiated with respect to levels of urbanization and economic development, Berry proposed a causal argument in which progress towards a lognormal ("rank-size" for Berry) distribution would be the result of economic development, larger area, and longer urban history. Yet he maintained that these causes operate in an essentially stochastic manner:

> *The model in effect proposes a major hypothesis: increasing entropy is accompanied by a closer approximation of a city size distribution to lognormality.* Common sense leads to several sub-hypotheses: *fewer forces will affect the urban structure of a country (a) the smaller is that country, (b) the shorter is the history of urbanization in the country, and (c) the simpler is the economic and political life of the country and the lower its degree of economic development. The converse of each sub-hypothesis also follows (Berry 1961:584; emphasis added).*

Though multiple causes generate the rank-size distribution, Berry tries to indicate the singular value of his sub-hypotheses by selecting illustrative examples. He states:

> *The limited data available from the previous discussion seem to bear out several of the sub-hypotheses. Countries with rank size distributions include urban-industrial economies (Belgium, United States), larger countries (Brazil), and countries with long histories of urbanization (India and China) . . . Primacy [dominance by a major city] characterizes small countries with simple subsistence economies (Thailand) or is associated with the presence of an empire capital (Portugal) (Berry 1961:584).*

But Berry's own data can be used to flatly contradict this *ad hoc* theorizing. For example, rank-size distributions include countries *lacking* an urban-industrial economy (El Salvador, India, China), small countries (Finland, Belgium, Switzerland) and countries with short histories of urbanization (El Salvador, Brazil). The term stochastic apparently gives one license to "theorize" *post hoc* at will. Again, the assumption of universal causal order would have to be abandoned if one were to hypothesize that, say in the cases of China and the United States, *different* economic and social processes had generated the *same* rank-size distribution of cities. But Berry is searching for universal *causal* forces within the context of a universal *stochastic* model. Ultimately, as Higgs (1970:273) has disparagingly noted, "explanations hinging upon unnamed 'forces' which are 'many and act randomly' [as in a stochastic model] are not theories at all but rationalizations for the lack of a theory." Labelling the forces is no improvement.

Since the Berry and Garrison article was published, and although Berry's 1961 article presented evidence to the contrary, many geographers accepted the rank-size rule and its associated "theory" as a "normal" model of city-size distributions. Berry reiterated the orthodoxy of treating the stochastic as explanation when, in 1964, he wrote:

> *For all large, complex systems of cities which exist in the world . . . aggregate growth patterns do conform to . . . a stochastic process, so that one macroscopic feature of these systems is a rank size regularity of city sizes. This regularity may, in turn, be "explained" by the stochastic process (Berry 1964:150).*

The uncritical acceptance of this perspective on the rank-size rule and the reasoning process it entails is indicated by a review of writing on city-size distributions in human geography. Table 2 shows, using journal articles, the extent to which the Berry perspective dominated research by geographers on city size distributions. Of thirteen articles, six make reference to Simon's 1955 paper and another four refer to Simon's work by noting Berry and Garrison's acceptance of it. Only four authors cite Beckman's (1958) attempt to integrate central place theory and the rank-size rule. All reject his "deterministic" approach in favor of the Simon model (also see Beckmann 1968; Beckmann and McPherson 1970). Other writers who have addressed problems associated with describing and explaining city size distributions (Jefferson 1939; Zipf 1949; Stewart 1947) are treated as historical precursors who now have little to offer. This may reflect Berry and Garrison's (1958) decision to opt for what they termed the "simplest alternate," Simon's stochastic model. "Simplicity" again appears as a major "theoretical" principle.

There are no attempts in the geographical literature to review and criticize the various plausible theories of rank-size and other city size distributions comparable to work by the economist Richardson (1973). Geographers interested in city-size distributions have had no time for competing theories. They apparently prefer an "accepted" theory, however inadequate.

TABLE 2 NUMBER OF CITATIONS TO PREVIOUS ARTICLES IN PUBLICATIONS ON THE RANK-SIZE RULE, 1965-1974

PREVIOUS ARTICLE CITED	NUMBER OF CITATIONS
Berry (1961)	9
Simon (1955)	6
Berry and Garrison (1958)	5
Beckman (1958)	4
Stewart (1947)	4
Bell (1962)	2
Berry (1964)	1
Friedmann (1963)	1
Haran and Vining (1973a)	1
Mehta (1964)	1
Parr (1969)	1
Rosing (1966)	1
Thomas (1967)	1

Sources: After Bain (1977) and Berry (1964), Boal and Johnson (1965), Davies (1972), Fowler (1972), Hodgson (1972), Haran and Vining (1973a, 1973b), Parr (1969), Rose (1966), Rosing (1966), Thomas (1967), Vapnarsky (1969), Vining (1974).

Berry's (1961) study of the causal forces "buried" in the stochastic model is the most referenced work across the thirteen articles. Nine authors cite and accept Berry's *ad hoc* theorizing. Most of the empirically based articles provide their own versions. For instance, Davies (1972:48), in a study on the Ontario-Quebec (Canada) urban system, states that:

> *Lognormal distributions . . . are essentially the result of stochastic growth processes, with distortions from lognormality the result of barriers to the operation of many forces within the system. In Ontario and Quebec, these barriers are hypothesized to include the historical effects of colonial rule, which tended to produce dual primacy, such as that of Toronto and Montreal; the effects of an economic system which is partially protected from American competition and thus from certain institutional differences which have in the past encouraged separate development in the two provinces. . .*

Among the authors cited, only Rose (1966) and Rosing (1966) questioned the utility of the rank-size rule, and Rosing was concerned with the accuracy of empirical findings, rather than the lack of any satisfactory theoretical rationale.

If we turn our attention to introductory human geography textbooks, further evidence of the emergence of an accepted wisdom on city-size distribution is manifested. The overt acceptance of Simon's stochastic model in five of thirteen major textbooks and the approving citation of Berry's "sub-hypotheses" on causal forces in six of them, underscores the extent to which Berry's perspective is presented as established knowledge to the beginning undergraduate (see Table 3). Of those authors who treat the rank-size rule, only Haggett (1966, 1975), Kariel and Kariel (1972), and King and Golledge (1978) suggest that the perspective may be lacking. Haggett hints at the inapplicability of the rank-size rule at the lower end of the urban hierarchy. Kariel and Kariel, and King and Golledge, mention the lack of "conclusive evidence" for *substantive* explanation of the rank-size rule.

Why Did Berry's Perspective Persist?

The persistence of Berry's perspective on the rank-size distribution may be accounted for by two major factors: a preference for arguments that suggest universal order and the disciplinary game of "follow the leader."

Throughout Berry's own writing there is an explicit and recurrent commitment to geography as a nomothetic science. Faced with empirical occurrences which do not concur with "lawful" regularity, it is reality that has to give way. Further, the rank-size rule was imbued with certain universal processual underpinnings from which universal *causal* order could be claimed (the self-fulfilling prophecy). This universal order was not based upon the careful accumulation of substantive processual evidence, but upon the *inference* of process from patterns in city-size distribution data correlated with gross national characteristics.

That the explanation was weak and based on *ad hoc* theorizing seems not to have impressed those who followed in Berry's footsteps. They, too, were enamored of the rank-size rule's apparent "lawfulness", the tie-in with Berry's notion of an entropy model and alleged ties to General Systems Theory; and, perhaps, in those heady days of the 1960's, the *lack* of regional or local content. After all, examining specific places would smack of the denigrated "idiographic" approach.

We suggest, moreover, that claims to universal order are maintained on sociological as well as intellectual grounds. With respect to the rank-size rule, there appears to have been such a persistent attachment to Berry's (1961) perspective that the research involved conformed to a game of "follow the leader." During the 1960's Brian Berry

TABLE 3 STATUS OF THE RANK-SIZE RULE AND ASSOCIATED STOCHASTIC MODELS IN INTRODUCTORY HUMAN GEOGRAPHY TEXTBOOKS

TEXTBOOK	DISCUSSION AND ACCEPTANCE OF SIMON'S MODEL	DISCUSSION AND ACCEPTANCE OF BERRY'S (1961) INTERPRETATION OF SIMON'S MODEL
Alexander (1963)	no discussion	no discussion
Berry and Horton (1970)	yes	yes
Berry, Conkling and Ray (1976)	no discussion	no discussion
Broek and Webb (1968)	no discussion	no discussion
Carter (1972)	no discussion	no discussion
Garner in Chorley and Haggett (1967)	yes	yes
Cox (1972)	no discussion	no discussion
Haggett (1966)	yes	yes (with proviso)
Haggett (1975)	yes	yes (with proviso)
Kariel and Kariel (1972)	no discussion	yes (with proviso)
King and Golledge (1978)	no discussion	yes (with proviso)
Lloyd and Dicken (1977)	no discussion	yes
Yeates and Garner (1971)	yes	yes

Source: After Bain 1977.

certainly attained status as a leader of the "new wave" within human geography.

For example, the research of Bell (1962) is indicative of a commitment to the academic establishment. In her study of "Change in City Size Distribution in Israel," Bell graphed the city size distributions of "Israeli" cities for every decade in the period 1922 to 1959 and, seeing a shift upward in the lower end of the curve, concluded that:

> *The 1944 and 1959 curves would then seem to follow the changes suggested in Berry's developmental model towards a rank-size order. During this time, Israel, although a very small nation, has been developing into a highly complex industrial one (Bell 1962:103).*

However, this conclusion may be purely coincidental. Five of the thirteen cities used for the 1922, 1931, and 1944 distributions were not included for 1959 as they were cities within the boundaries of Egypt and Jordan rather than the new state of Israel after 1948 (Bain 1977). The lack of a consistent set of cities over all the distributions clearly limits the efficacy of making historical inferences. In this case, as in others (see Rose 1966), the form of national city size distributions seems to depend upon the definition and stability of political boundaries. Bain argues that:

> *If one examines the evolution of city size distributions within a historical context the question of the territorial stability of the national entity is frequently called to account. Had Colonel Ojukwu received greater support from world powers, the emergence of Biafra would have altered the city size distribution of "Nigeria" drastically (Bain 1977:79).*

Rather than viewing the shift of the lower end of the city size distribution as a result of "development," Bell could have noted the deliberate government policy after 1948 of developing "new town" growth centers which had the effect of increasing the number of cities in the smaller city size classes (Bain 1977). However, Bell apparently committed herself to Berry's perspective on the rank-size rule without much concern for the *real* situation in the Palestine area over the period 1922-1959. Her findings pleased Berry. He wrote in 1964 that Bell's study, along with that of Friedmann (1963), provided additional evidence that ". . . increasing complexity of a space economy certainly brings the city size distribution closer to rank size" (Berry 1964:149-150).

Mathematical functions describing city size distributions, of which the rank-size rule is one, are sufficiently similar that the differences between them are inadequate for any one to be determined as "true." Moreover, since any one of the mathematical functions is compatible with several theories of city-size distribution (Richardson 1973), it is unclear how one can use such functions for theoretical purposes. This is essentially the same problem we encountered with the instrumentalist/realist confusion over the meaning of S-shaped curves in diffusion research.

For a considerable number of years many geographers have insisted on using one mathematical function, that of the rank-size rule, and offering a consistently *ad hoc* substantive explanation of its genesis, to account for *all* city-size distributions. We have argued that this acceptance of one perspective was based on total commitment to a universal order "revealed" by data manipulation. That statistical order and its causes might be based upon detailed comparative studies to evaluate competing theories was never seriously considered.

7

Order and Skepticism

The search for order in reality is a fundamental concern of scientists; indeed, it might be said that the principle stands supreme above all others in the scientist's mind. Moreover, those who disavow any association with science or refer to themselves as humanists even accept that discovering pattern, reason or, simply, order is their ultimate quest.

For some scientists the search for order borders on being an obsession; they are not above stretching method and a known moral code to obtain desired ends. They do so by over-simplification of a complex process, by selectively choosing and sifting data, by not allowing data to get "too close" to their model or theory, by setting up hypotheses and research designs in ways that produce results originally imagined, and by accepting models of reality that leave little or no room for genuine falsification. In all of these matters geographers participate.

The way in which order is paired with skepticism is of paramount importance. Although there is every reason that the two should be tightly interrelated, and although it is certain that some scholars appreciate this deceptively simple point, it is clear that for many scientists, and even for some disciplines, the interplay between the search for order and skepticism/criticism is not what it should be. We have accused human geography (with special attention to its positivist practitioners) of being in this predicament and have offered three case studies as evidence.

One might wish to take issue that all three of our extended examples deal with "old interests," not emergent paradigms and revisionisms that, for some at any rate, sparkle on the horizon, fruit soon to be harvested. In our view this kind of objection is misplaced. Skinner's claims in regard to market areas and what we criticize in interpretations of S-shaped curves and the rank-size rule remain as widely disseminated knowledge. Uncriticized propositions are widely represented in textbooks, cited in numerous professional articles to this day and, in some instances, are integral to continuing research. The simple fact is that the dialectical relationship between order and skepticism, of the sort we have outlined, has not been taken seriously by most geographers. But there is another matter. Had we chosen more recent examples we would have left ourselves open to the charge that too little time has elapsed between publication of the ideas and their dissemination into the social science community. Finally, whatever one might wish to conclude about more recent concerns for structuralism, humanism, Marxism, and other *isms*, it would be both foolish and naive to believe that any one or combination of

these alternatives will do away with positivism or, more generally, naturalism in geography (Johnston 1979), or that new or revitalized paradigms will make the order-skepticism dialectic somehow superfluous. The dialectic is as applicable to newer trends and works in geography as much as it is to the excesses of the 1960's or the entrenched parochialism of an earlier period. In this regard, one only need cite those Marxists who continue to confuse a *model* of capitalism with actual capitalist societies. Geographer Marxists, like humanists and others who seek alternatives to a positivist model of science, write about the need for examining one's premises and, equally, the need for criticizing one's conclusions. But, with few exceptions, they have failed to be profoundly critical of their own models, not to be confused with their criticism of others' models (Agnew and Duncan 1981). They have not incorporated the model of inquiry we have discussed into their own philosophies. Humanists and Marxists can profit from the order-skepticism dialectic.

Derek Gregory in *Ideology, Science and Human Geography* (1978) has been most outspoken in recent years about the need for a "critical geography." Unfortunately, his message takes us not much further than did the long list of humanistic pronouncements of the last decade (Buttimer 1974; Ley 1977). All have been long on asserting what ought to be done and have tended to focus on programmatic statements rather than actual research practice. With a few notable exceptions, those who enjoy their role as philosopher have proved impotent in demonstrating the substantive or theoretical implications of their message (but see Guelke 1976). The Marxists are less of an exception than one might think, for, unfortunately, their practical demonstrations have been rather few in number. That this is not necessarily intrinsic to Marxism is clear from the work of many Marxist historians. But there is another issue here, one to be taken up with Gregory and those who have accepted his clarion call. He wants a "critical social science," therefore a "critical geography." But, as we have argued, this is a fundamental misunderstanding of the nature of scholarly activity. Skepticism and criticism, like the search for order, should be intrinsic to social science analysis. They are not something apart from it, an epistemological category that needs to be incorporated later.

Some readers who accept a definition of scientific reasoning akin to that which is criticized in this book may see our argument as "anti-scientific." This would be a mistake. Our concern is with denying the efficacy of a specific practice of science rather than science *in toto*. Our view of science is one that stresses the primacy of theory rather than data. Facts do not "speak for themselves," they are read in the light of theory. A theory is an explanation focused on reasons and causes (Toulmin, 1970), *not* a device for codifying data patterns. Consequently our position is not instrumentalist, relativist, or idealist (Winch 1963) but realist (Harré 1961, 1970, 1972; M. Bunge 1964). We do not deny science but desire to define it differently from the previous consensus (Amedeo and Golledge 1975). Our definition corresponds closely to the *actual practice* of much work in the natural sciences (developing theories as claims about reality — Harré 1972; Keat 1971; Keat and Urry 1975), if not to the *paradigm of science* proposed under the logical empiricist program widely accepted in the social sciences (Gunnell 1975) and criticized here. Order, then, is not necessarily something we invent. But we need skepticism to help ensure that it is not.

More specifically, we accept the proposition that facts are selective assertions about reality and, further, that the complexity of reality is unknowable in the absence of epistemological norms that designate principles for selecting facts (Weber 1947; 1975). Thus we see a necessity for skepticism in considering order-claims since epistemologi-

cal norms determine the definition of facts. However, it does not follow that one has a relativist account of theory. Rather, in Weberian fashion, "Causal analysis is subject to verification and is not affected by the vicissitudes of value-relevance" (Zaret 1980:1186; also see Ricoeur 1979:145-147). While accepting that there is a *true* reality, we would stress the need to compare and contrast theories through reason and observation to arrive at *true* understanding. *All theories are, therefore, provisional.*

A fundamental element in the argument of this book, however, is the necessity to treat the social, as well as the epistemological, context of research when constructing a critique of current practice. Our realism therefore is "moderate" rather than "extreme" by dint of recognizing the social context of research (Jardine 1979). Previous critical commentary in human geography, with its focus on proposing a "new" program at the same time as undermining an old one (Harvey 1969; Gregory 1978), has largely neglected the social context of research in order to pursue and perfect an intellectual edifice that will inspire future research. Yet, as much contemporary work in the philosophy of science reminds us, the social dynamics of scientific communities and the problem of personal commitment to theories interfere with any purely epistemological account of knowledge acquisition and questioning (Campbell 1979; Haack 1979). Rather than accepting the barriers to an order-skepticism dialectic posed by these contextual factors, however, we ask for their public recognition and hope for their progressive removal.

Textbook and research dogma has led human geography into an intellectual impasse over the status of "scientific" reasoning in the field. On the one hand, some who reject what has passed for science in the field propose a return to descriptive regional geography or put forward some form of "idealism" (Guelke 1974; 1977; Ley 1977). This reaction, however, is based on equating science with positivism. On the other hand, science has been defended largely in logical empiricist or positivist terms. Hence, the impasse concerning the status of human geography as science.

It is our hope, and our intention, that this book offers a preliminary critique of research and textbook practices in contemporary Anglo-American human geography. We do not present a full-blown "alternative," because skepticism and criticism should be inherent in our discipline whatever the *specific* philosophy of research that is adopted. However, as indicated previously, we do believe more explicit concern with theories would help direct attention to the foundations upon which claims of order rest and thus encourage skepticism and criticism. Future research can avoid the pitfalls of the past if it utilizes this viewpoint. We can do no better than end by noting that Mario Bunge (1964:viii) grasped the core of our thesis:

> *Criticism regarded as a minor or even shameful activity in some highly production-minded and success-oriented quarters, is the supervisor of creativity. . . . Reason proper is constantly testing its own creations as well as those of intuition; and testing is criticizing.*

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